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ABSTRACT

In order to measure their effectiveness in promoting rapid re-employment and shorter spells during which claimants are entitled to collect unemployment insurance (UI), the U.S. Department of Labor tested these three assistance strategies in the District of Columbia (8,071 claimants selected during June 1995-June 1996) and Florida (12,042 claimants selected during March 1995-March 1996): (1) Structured Job Search Assistance (SJSA), which required orientation, testing, job search workshops, and a one-on-one assessment interview; (2) Individualized Job Search Assistance (IJSA), which provided services based on needs of claimants; and (3) Individualized Job Search Assistance with Training (IJSA+), which provided the services of IJSA plus training provided by other agencies for interested claimants. Claimants were assigned at random to one of the three treatment groups or to a control group. Among the findings are the following: (1) compared to the control group, all treatments reduced UI receipts for the first year, with the largest impact occurring in the Structured Job Search Assistance group in the District of Columbia, where it was shortened by more than a week; (2) no treatments reduced UI receipt beyond the first year; (3) the researchers were not able to definitively answer which treatment was most effective in reducing UI spells; (4) the treatments had uneven impacts on employment and earnings; and (5) compared to the control group, all treatments led claimants to contact more employers per week in their job search. (CML)

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Assisting Unemployment Insurance Claimants: The Long-Term Impacts of the Job Search Assistance Demonstration

February 2000

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At Battelle, Terry Johnson, as co-principal investigator, played a key role in designing the demonstration and providing feedback during the evaluation. Jutta Joesch performed the analysis of data on service participation, customer satisfaction, and the validity of eligibility criteria.

Paul T. Decker
Project Director

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EXECUTIVE SUMMARY

BACKGROUND

The Emergency Unemployment Compensation Act of 1991 authorized the U.S. Department of Labor (DOL) to conduct the Job Search Assistance (JSA) demonstration to test the feasibility of implementing job search assistance programs and measure their effectiveness in promoting rapid re-employment and reduced UI spells among Unemployment Insurance (UI) claimants. The demonstration was designed to identify UI claimants, at an early point of contact with the UI system, who were likely to face lengthy UI spells and to provide them with assistance in finding a new job. Three different assistance strategies, which are described below, were tested in the demonstration. In each case, assistance was provided in the demonstration by the local Job Service (JS) agencies, while the UI agencies monitored participation in the demonstration and sanctioned claimants who failed to comply with the demonstration's participation requirements.

The demonstration was conducted in the District of Columbia (D.C.) and Florida, which were chosen based on plans submitted to DOL. The D.C. demonstration operated in a single office and served a targeted sample of claimants from the full D.C. claimant population. Claimant selection occurred between June 1995 and June 1996, and a total of 8,071 claimants were assigned to the demonstration. The Florida demonstration operated in 10 local Jobs and Benefits offices scattered throughout the state. Each local office served a targeted sample of claimants from the local UI claimant population. Claimant selection occurred between March 1995 and March 1996, and a total of 12,042 claimants were assigned to the demonstration.

DESIGN OF THE DEMONSTRATION

The demonstration tested three service strategies for promoting rapid re-employment and reduced UI spells among targeted UI claimants:

1. ***Structured Job Search Assistance (SJSA).*** Claimants assigned to this treatment were required to participate in an orientation, testing, a job search workshop, and a one-on-one assessment interview. Claimants who failed to participate in any service, unless explicitly excused, could be denied benefits. After completion of the services, claimants were required to have two additional contacts with demonstration staff to report on their job search progress.

2. ***Individualized Job Search Assistance (IJSA).*** This treatment assigned claimants to services based on their assessed needs. All claimants were required to participate in an orientation and a one-on-one assessment interview. During the assessment interview, the claimant and a demonstration staff member developed a service plan to address the claimant's needs. If the service plan included demonstration-specific services, such as testing, a job search workshop, or additional counseling, these services would become mandatory.

3. ***Individualized Job Search Assistance With Training (IJSA+).*** This treatment was identical to the second treatment, except for the inclusion of a coordinated effort with local Economic Dislocation and Worker Adjustment Act (EDWAA) staff to enroll interested claimants in training. During the orientation, an EDWAA staff member discussed local opportunities for training. Training opportunities were also discussed during the assessment interview, and any claimant interested in training was scheduled to meet with an EDWAA staff member at the demonstration office.

The demonstration services were intended for claimants with the greatest need for assistance–those expected to face long unemployment spells. Eligibility for the demonstration was determined through a two-stage process designed to identify such claimants. In the first stage, a series of characteristic screens was used to exclude claimants for whom JSA services were inappropriate, including claimants with an expected date of recall to their previous employer and those using union hiring halls. In the second stage, the probability of UI benefit exhaustion was estimated, based on a regression model, for each of the claimants passing the screens in the first stage. Among those who passed the screens, claimants with the highest exhaustion probabilities were targeted for the demonstration.

Claimants targeted for participation were assigned randomly to a control group or to one of the three treatment groups. Random assignment ensures that the treatment and control groups exhibit similar characteristics and that each is representative of the target population. In this demonstration, random assignment also allowed the differences in outcomes between each of the treatment groups and the control group to be interpreted as unbiased estimates of the net effects of the three service packages.

IMPLEMENTATION OF THE DEMONSTRATION

The demonstration was, for the most part, successfully implemented in both D.C. and Florida. Both states were successful in using the two-stage selection process to target the demonstration services to claimants likely to face long UI spells. Claimants selected as eligible for the demonstration but denied services (the control group) had longer average UI spells and were more likely to exhaust their benefits than claimants determined to be ineligible for the demonstration. In Florida, for example, the benefit exhaustion rate was about six percentage points higher for the demonstration-eligible claimants than for the ineligible claimants. In D.C., the exhaustion rate was about 13 percentage points higher for the eligible claimants. The differences in average UI spells between eligible and ineligible claimants was two weeks in Florida and 1.5 weeks in D.C. These differences are not huge, but they are probably what would be expected from a statistical model of the determinants of benefit exhaustion among UI claimants.

Both states generally offered the services as they were designed for each of the treatments. Claimants assigned to SJSA were offered a set of mandatory services, including an orientation, testing, job search workshop, and an assessment. The majority of claimants assigned to the demonstration attended at least the orientation, and the majority of those attending the orientation also attended testing, the workshop, and the assessment. Most of the claimants who failed to attend a service did so because they had become reemployed and/or had stopped collecting UI benefits.

Claimants assigned to IJSA and IJSA+ were also offered the full set of services, but few of these claimants participated in any JSA group services other than the orientation and assessment. Orientation and assessment were the only mandatory services for all claimants assigned to IJSA and IJSA+ who wanted to continue collecting benefits. These claimants were required to participate in the other JSA services–testing and the workshop–only if these services were part of the individual service plan created as part of their assessment interview. Few claimants in either state participated in testing or the workshop. Attendance was especially low in D.C., where less than 1 percent of claimants who attended orientation also attended testing or the workshop. The corresponding attendance rates in Florida were higher but still modest–in the 10 to 20 percent range.

The low attendance rates for testing and the job search workshop in the IJSA and IJSA+ treatments suggest that demonstration claimants were reluctant to participate in services that were not universally mandatory. Furthermore, although IJSA and IJSA+ claimants were offered testing and the workshop, JSA staff were reluctant to make these services mandatory. Presumably, caseworkers either felt the services were inappropriate for most claimants or did not want to jeopardize claimants' benefits by requiring participation. Given this, the services received by SJSA claimants, who were automatically required to participate in all the group services, clearly exceeded those received by the IJSA and IJSA+ claimants.

To generate substantial rates of participation in group services, an ongoing program would probably need to make these services universally mandatory. Claimants are reluctant to volunteer for services, as has been shown in this and other demonstration evaluations. In addition, staff are unlikely to aggressively assign claimants to services. Given the reluctance of both claimants and staff to initiate service participation, the only way to ensure participation is to make the services mandatory.

D.C. emphasized individual counseling rather than group services for IJSA and IJSA+ claimants. Nearly half of the claimants assigned to IJSA or IJSA+ were reported to have participated in counseling. The emphasis on individual counseling in D.C. may have arisen because of the limited space and trained staff available to conduct group services in the D.C. office.

The timing of JSA services was consistent with the demonstration design, which was based on the objective of achieving early intervention. During the design phase of the demonstration, it was determined, given the time needed to identify and notify claimants, that services would ideally begin about 7 weeks after the initial UI claim. Our findings on timing of participation show that the demonstration generally achieved early intervention according to this standard--the average time from the beginning of the benefit year to orientation was about 7 weeks in both states, and about 80 to 85 percent of claimants participated in orientation within 8 weeks of the beginning of the benefit year. Most claimants also moved on quickly to subsequent services. Claimants assigned to SJSA typically finished all services by the end of the second full week after their orientation. Claimants assigned to IJSA or IJSA+ usually completed assessment within a week of their orientation.

Data on EDWAA training show that few demonstration claimants, even those assigned to IJSA+, participated in EDWAA training. However, the training rate was higher among the combined treatment groups than in the control group. In Florida, the training rate was 3.5 percent for the combined treatment groups compared with 2.8 for the control group, while in D.C. the corresponding training rates were 1.3

percent and 0.8 percent. These numbers imply that the information provided through orientation and assessment, the services offered in all treatments, was effective in inducing claimants to participate in EDWAA training. The effect, however, was fairly small and the resulting rate of training participation was also small.

The training rate was, however, no higher among the IJSA+ groups than among the other treatment groups, so we conclude that the IJSA+ approach was not any more effective than SJSA or IJSA in providing greater access to training. There were at least two reasons for this outcome. First, demonstration claimants were not treated as being automatically eligible for EDWAA, as was expected when the demonstration was designed. In most sites, before claimants could enter EDWAA training, they had several eligibility or procedural hurdles to clear, which greatly impeded their potential entry into training. Second, coordination between the local demonstration sites and EDWAA often fell short of our expectations. EDWAA staff did not always participate in the demonstration services as they were designed, so in some local offices IJSA+ provided no greater contact with EDWAA than the other treatment groups.

Based on the findings from previous JSA demonstration reports, we know that both states monitored and enforced the JSA participation requirements, but staff in the two states differed in their attitude and approach. Demonstration staff in both states told claimants that participation in the demonstration was mandatory and that claimants could lose their benefits if they refused to participate, but staff in Florida tended to downplay these aspects of the demonstration. Although staff in both states contacted claimants who failed to attend required services, D.C. tended to be more rigorous than the Florida sites in enforcing the requirements. In D.C., claimants who missed a single service were sent a noncompliance notice instead of their UI check, and they were required to report to the demonstration office to meet with a claims examiner to collect their benefit check. In contrast, most Florida offices allowed no-shows to maintain their benefits and reschedule missed services over the phone rather than by reporting to the demonstration office in person. Benefit checks in Florida were held up only if claimants missed multiple appointments.

These findings demonstrate that states are likely to enforce similar participation requirements very differently. States will come to different decisions about what constitutes noncompliance and how to warn claimants that they are at risk of losing benefits.

IMPACTS OF THE JSA DEMONSTRATION

We estimated impacts of each of the demonstration treatments on various measures of UI receipt, benefit nonmonetary determinations and denials, employment and earnings, job characteristics, and job search activities. The treatments were expected to increase search effort, speed re-employment, and reduce UI benefits.

Impacts on UI Receipt and Eligibility

Each of the JSA treatments reduced UI receipt in the initial benefit year (year 1). The largest impact occurred in the SJSA group in D.C., where UI receipt was reduced by more than a week, as shown in

Table 1. The other treatments in D.C. and all three treatments in Florida had more modest impacts, reducing UI receipt by about half a week. The treatments also reduced the percentage of

TABLE 1

ESTIMATED IMPACTS OF THE JSA TREATMENTS ON UI RECEIPT

	District of Columbia			Florida		
Outcome	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
Year 1 UI Outcomes[a]						
Weeks of UI Benefits	-1.13***	-0.47**	-0.61**	-0.41**	-0.59***	-0.52**
Rate of UI Benefit Exhaustion (Percent)	-4.8***	-2.4*	-3.9***	-1.8*	-2.4**	-2.8**
Percent with at Least One Nonmonetary Benefit Determination	36.6***	29.0***	28.7***	4.4***	2.7**	2.8***
Percent with at Least One Nonmonetary Benefit Denial	10.8***	8.1***	7.0***	2.9***	3.0***	2.0***
Year 2 UI Outcome[b]						
Weeks of UI Benefits	0.12	-0.15	-0.06	0.11	0.03	0.17

[a]Year 1 is the initial benefit year.
[b]Year 2 includes all UI receipt resulting from an initial claim filed within one year of the end of year 1.

*Statistically significant at the 90 percent level in a one-tailed test.
**Statistically significant at the 95 percent level in a one-tailed test.
***Statistically significant at the 99 percent level in a one-tailed test.

claimants who exhausted their benefits, with the estimated reduction ranging from about 1.8 to 4.8 percentage points. In studying the timing of the impacts on UI receipt, we found that the treatment-control differences in UI exit rates occurred early in UI spells, around the time that claimants were notified of JSA service requirements or when they would have been scheduled to participate in services. This finding implies that much of the impact on UI receipt is due to an immediate response to the participation requirements or the services rather than to a gradual application of the skills learned during program participation.

None of the treatments had a significant impact on UI receipt beyond the initial benefit year. This finding is consistent with our expectations--we expected the treatments to help claimants become re-employed more quickly but not to have an effect on longer-term job stability. At the same time, our findings are inconsistent with those from the New Jersey UI Re-employment Demonstration, which showed that a structured JSA package generated a significant reduction in UI receipt in the second year after the initial claim.

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These findings do not allow us to draw a definitive conclusion about which service strategy is most effective in reducing UI spells. In D.C., SJSA generated a larger reduction in UI spells than did IJSA and IJSA+. But in Florida, the impact of SJSA was nearly identical to the impacts of IJSA and IJSA+. Given these findings and our information about the enforcement policies in the two states, we conclude that the SJSA service approach is likely to generate larger UI reductions in settings where the additional participation requirements associated with SJSA are strictly enforced. Another factor that may have contributed to the large impact of SJSA in D.C. is that the D.C. control group had long UI spells, so there was the potential for substantial reduction in UI spells in response to the demonstration. The SJSA approach implemented in D.C. may be an effective strategy for realizing this potential.

All of the JSA treatments increased nonmonetary benefit eligibility determinations and denials in year 1. Table 1 shows that the treatments in D.C. increased the rate of determination by 29 to 37 percentage points and the rate of denials by 7 to 11 percentage points, depending on the treatment. The impacts on determinations and denials in Florida tended to be smaller but still substantial. Most of the increases in determinations and denials were related to regular UI benefit eligibility issues, not to JSA participation directly. It appears that local staff used the information gathered through the demonstration to more strictly enforce traditional UI eligibility requirements for claimants assigned to the treatments. The increase in benefit denials was responsible for part but not all of the impact of the treatments on UI receipt, especially in D.C., where the increase in benefit denials was largest.

Impacts on Employment and Earnings

The JSA treatments had somewhat uneven impacts on employment and earnings following the initial UI claim. On the one hand, the SJSA group in D.C. generally had higher earnings than the control group, and the differences tend to be statistically significant. Moreover, the impacts of SJSA on quarterly earnings in D.C. were fairly large, about $200 per quarter, and persistent over the 10- quarter follow-up period (see Table 2). On the other hand, the estimated impacts of SJSA on quarterly earnings in Florida and the estimated impacts of IJSA and IJSA+ on quarterly earnings in both states tended to be smaller (often even negative in Florida) and not statistically significant in most cases. However, both IJSA and IJSA+ significantly increased quarterly earnings in D.C. during or shortly after the initial benefit year. The impacts on employment rates (not shown in Table 2) were similar to the impacts on earnings.

We found no evidence that the treatments pushed claimants into lower–quality jobs in order to hasten their re-employment. On the contrary, the treatments appear to have potentially improved the quality of the jobs accepted by participants. The treatments also did not affect the likelihood that claimants would switch occupations when they accepted a new job.

TABLE 2

ESTIMATED IMPACTS OF THE JSA TREATMENTS ON EARNINGS
(Dollars)

	District of Columbia			Florida		
Quarter[a]	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
1	30	22	22	53	-48	-24
2	172**	102	147**	-4	-6	20
3	152***	111	176**	-53	-18	14
4	281***	161**	83	-2	122	50
5	280***	191**	180**	-92	-36	-12
6	241**	183**	106	-66	-36	5
7	177*	96	-23	-57	-5	63
8	263**	129	38	-98	-41	-20
9	185*	76	10	-98	-41	-49
10	224**	100	50	-23	-30	-44
11				-33	6	14
12				-121	50	9

[a]Full calendar quarters following initial UI claim.

*Statistically significant at the 90 percent level in a one-tailed test.
**Statistically significant at the 95 percent level in a one-tailed test.
***Statistically significant at the 99 percent level in a one-tailed test.

Impacts on Job Search

The JSA demonstration encouraged more aggressive job search efforts among treatment group members. In both D.C. and Florida, each of the JSA treatments led claimants to contact more employers per week in their job search, as shown in Table 3. For example, SJSA treatment generated 1.6 and 1.4 additional contacts per week in D.C. and Florida, respectively. The IJSA and IJSA+ treatments also increased the number of employers contacted as well as the hours spent searching for work.

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TABLE 3

ESTIMATED IMPACTS OF THE JSA TREATMENTS
ON JOB SEARCH

Outcome	District of Columbia			Florida		
	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
Employers Contacted Per Week	1.6*	1.9*	3.0*	1.4**	1.5*	2.1**
Hours of Search Per Week	0.2	0.6	0.9	0.4	0.7	1.7**
Percent Receiving a Job Referral from the Job Service	8.7***	2.9	8.7**	3.4*	3.8	10.3***

*Statistically significant at the 90 percent level in a one-tailed test.
**Statistically significant at the 95 percent level in a one-tailed test.
***Statistically significant at the 99 percent level in a one-tailed test.

Another expected impact of the demonstration was to increase claimants' use of the state Job Service (JS) to assist in their job search. All of the JSA treatments increased contact with the JS as designed, and they also tended to increase the probability that claimants received job referrals from the JS, as shown in Table 3. However, we found no evidence of impacts on the likelihood of obtaining job offers through JS referrals. Thus, it appears that the JSA demonstration was successful in getting claimants to use the JS, but that it was less successful in matching claimants to job offers generated through the JS. Regardless, the expanded use of JS may have helped claimants find jobs on their own.

COST-EFFECTIVENESS OF THE JSA TREATMENTS

Table 4 shows estimated costs per claimant, benefits per claimant, and the implied rate of return for each treatment from the perspectives of DOL, the government as a whole, and society as a whole. The cost estimates in the first panel of the table reveal that, as expected, the individualized JSA treatments were less costly than the structured treatment. The costs are constant across the three perspectives since all demonstration costs were incurred by DOL, and DOL costs are also a subset of both total government costs and societal costs.

All of the JSA treatments yielded benefits for DOL primarily due to decreased UI payments. Most of the treatments also yielded benefits for total government. For society as a whole, the benefits estimates diverge substantially between D.C. and Florida. In D.C., the treatments yielded substantial benefits primarily due to the significant earnings increases caused by the treatments. In contrast, two of the three Florida treatments yielded negative benefits because our estimates suggest that the treatments reduced earnings (although the estimates are not statistically significant).

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TABLE 4

ESTIMATED COSTS AND BENEFITS OF THE JSA TREATMENTS

Perspective	District of Columbia			Florida		
	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
Costs (Dollars per Claimant)						
Department of Labor	286	199	216	241	97	103
Total Government	286	199	216	241	97	103
Society	286	199	216	241	97	103
Benefits (Dollars per Claimant)						
Department of Labor	160	89	31	17	97	47
Total Government	717	416	254	-110	77	54
Society	2,647	1,552	1,060	-763	-119	43
Rate of Return $(\frac{Benefits - Costs}{Costs})$						
Department of Labor	-44%	-55%	-86%	-93%	0%	-54%
Total Government	151%	109%	17%	-146%	-20%	-47%
Society	826%	680%	391%	-416%	-222%	-59%

The final step in our analysis of the JSA treatments was to combine the cost and benefit estimates to evaluate the cost-effectiveness of the treatments. In the final panel of Table 4 we present the estimated rate of return on the resources invested in each treatment, which is equal to net benefits (benefits-costs) divided by costs.

Our estimates imply that the JSA treatments were not cost-effective from the perspective of DOL. Table 4 shows that none of the treatments in either state generated a positive return on the resources invested by DOL--the estimated reductions in UI payments caused by the treatments were not large enough to fully compensate for the costs of the services. The best case scenario implied by our estimates is that DOL would break even on their investment in JSA.

Although the JSA treatments were not generally cost-effective from DOL's perspective, they may have been cost-effective from a broader perspective. The D.C. treatments generated substantial returns from the perspectives of government and society as a whole. For example, the societal rate of return for the

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SJSA treatment was 826 percent, which implies that one dollar invested in SJSA yielded $8.26 in benefits for society. In contrast, the same treatments in Florida failed to generate positive returns for either the government or society as a whole. We hesitate to draw strong conclusions on the returns to society based on the Florida findings, since these estimates are sensitive to the earnings impacts, which are imprecisely estimated. In Florida, for example, the negative returns are driven partly by the finding that the treatments negatively affected earnings, but these negative estimates are statistically indistinguishable from zero.

IMPLICATIONS FOR WORKER PROFILING AND RE-EMPLOYMENT SERVICES

The Unemployment Compensation Amendments of 1993 required states to develop Worker Profiling and Re-employment Services (WPRS) systems to identify UI claimants who might benefit from re-employment services and then refer them to re-employment services. These amendments directed all states to build their own statewide job search assistance systems. For WPRS, states are required to use the same two-step approach used in the demonstration to identify claimants to be referred to services. In most states, service referral in WPRS is similar to the IJSA treatment in the demonstration--each claimant is required to meet one-on-one with a counselor to develop an individual service plan and assess the claimant's interests and abilities (Dickinson et al. 1999). Most states in WPRS also require at least some claimants to participate in individualized services beyond the standard mandatory services. However, as in IJSA, the percentage of claimants in any state actually required to participate in additional individualized WPRS services may be fairly low.

The demonstration findings suggest that the typical WPRS service approach, which does not automatically require claimants to participate in services beyond orientation and assessment, is unlikely to generate widespread participation in other group services such as testing or job search workshops. To generate widespread participation, the states probably need to mandate these services. Findings from the WPRS evaluation presented in Dickinson et al. (1999) are largely consistent with this argument. Among the five states with valid data on service participation, the two states that explicitly required claimants to participate in a job search workshop as part of their WPRS requirements (New Jersey and Maine) generated fairly high workshop participation rates--about 40 percent or more. The other three states (Connecticut, Illinois, and South Carolina), which did not have explicit workshop requirements, generated much lower participation rates. Hence, it appears that in the early days of WPRS, substantial participation in many services was only achieved through explicit requirements that were backed up by the threat of benefit denials.

Recommendation: If states want to expand services received by claimants through WPRS, states should make particular services mandatory for all claimants referred to WPRS, or at least encourage local offices to be aggressive in using individual service plans to set and enforce service requirements.

Findings from the demonstration also suggest that coordination under WPRS between UI/JS and local agencies authorized to provide training under the Workforce Investment Act (WIA) may be difficult. In both of the JSA demonstration states, as explained above, demonstration staff had some difficulty in working with EDWAA staff and getting claimants into EDWAA training quickly. This is consistent with

early observations of the WPRS systems presented in Hawkins et al. (1995), which reports that in many of the subject states, EDWAA played little or no role in WPRS. The researchers argue that improved linkages between EDWAA and the local UI and JS agencies involved in WPRS would allow the agencies to take better advantage of EDWAA expertise in serving dislocated workers with diverse needs. Coordination between UI/JS and EDWAA may have improved over time. Based on responses to a 1997 survey, Dickinson et al. (1999) report that in 50 percent of states, EDWAA was substantially involved in at least one major WPRS task. Furthermore, EDWAA has now been replaced by WIA. The WIA requirement that local areas establish One-Stop Career Centers, which bring multiple agencies together in a single location to serve all clients, should contribute to improved coordination between UI/JS and the WIA agencies.

Recommendation: DOL should continue to develop new tools, in addition to the One-Stop Career Centers, to encourage coordination of UI/JS and WIA and increase the exposure of WPRS claimants to WIA services.

WPRS participation requirements are likely to increase UI nonmonetary benefit determinations and denials. Some of the increase will be due to direct enforcement of the WPRS requirements. But much of the increase will be due to more strict enforcement of traditional UI eligibility requirements. This kind of enforcement will be possible because of the additional information that local offices collect from claimants to track WPRS activities. Dickinson et al. (1999) confirm that WPRS increased nonmonetary benefit determinations and denials in most of the states that they examined.

The JSA demonstration findings suggest that WPRS generates modest reductions in UI receipt. According to our estimates, the IJSA treatments, which most resembled typical WPRS services, reduced UI receipt by about half a week. Estimates from the WPRS evaluation reported in Dickinson et al. (1999) confirm that WPRS has an impact on UI receipt. WPRS reduced UI receipt in four of the six states investigated by Dickinson et al., with estimated reductions in the four states ranging from one-quarter of a week to one full week of benefits.

Implications of the JSA demonstration findings for the impacts of WPRS on employment and earnings are more mixed. The IJSA treatments increased earnings in some quarters in D.C., but we found no clear evidence that the treatments increased earnings at all in Florida. Dickinson et al. also found no clear evidence that similar services in WPRS increased employment or earnings, even in the states where UI receipt was significantly reduced.

Finally, our findings provide little evidence that moving WPRS to a more structured model would be cost-effective. While in D.C. the rate of return on investment in SJSA was somewhat higher than on investment in IJSA, in Florida we found just the opposite. Furthermore, these comparisons are very sensitive to the earnings impacts, which are estimated imprecisely.

Recommendation: Structured services do not necessarily maximize cost-effectiveness. States should use structured services only if their primary objective in WPRS is to expand service participation.

I. INTRODUCTION

Increasing concern about dislocated workers has generated interest in finding effective and efficient methods for assisting these workers. The State Employment Security Agency (SESA) system, which is responsible for providing cash benefits and other re-employment assistance for unemployed workers in general, provides a setting in which dislocated workers can be identified and served early in their unemployment spells. SESA encompasses two agencies--the Unemployment Insurance (UI) system and the state Job Service (JS)--that play key roles in this process. Workers who lose their jobs through no fault of their own and are covered by Unemployment Insurance (UI) can file a claim for weekly UI cash benefits immediately after separation from their job. Following their initial claim, these workers, or claimants, are referred to the JS, which offers job placement assistance and other services to help claimants find new jobs.

The Job Search Assistance (JSA) demonstration was designed to take advantage of the early contact unemployed workers have with the UI system to identify permanently separated workers who were likely to face lengthy UI spells and to help them find new jobs. Early intervention was emphasized in an effort to maximize the potential reduction in average UI spells. The demonstration was also designed to test alternative strategies for reducing UI spells, with the strategies varying in the degree to which services were customized for each worker.

All of the service strategies tested in the JSA demonstration were designed to coordinate the efforts of the UI system, the state Job Service, and the local and state agencies providing services under the Economic Dislocation and Worker Adjustment Act (EDWAA). EDWAA services, which are intended specifically for dislocated workers, include classroom and on-the-job training, job

search assistance, and other reemployment services. By combining the efforts of these agencies, the demonstration was designed to offer a variety of services and address the claimants' varying needs.

A JSA demonstration in New Jersey during 1986 and 1987 indicated that the UI system could be used to identify dislocated workers and that re-employment services targeted at these workers could successfully reduce unemployment spells (Corson et al. 1989). After the success of the New Jersey demonstration, the Emergency Unemployment Compensation Act of 1991 (Public Law 102-164) authorized demonstrations to test this concept further in two additional states, Florida and the District of Columbia (D.C.).[1] By relying on an experimental design, the evaluation is able to measure how successful these services are at improving the employment outcomes of dislocated workers and reducing the burden on the UI system.

This report examines the participation of claimants in the Florida and D.C. demonstration projects and the impact of the demonstrations on claimant outcomes over approximately two years following each claimant's initial UI claim. This introductory chapter offers background about the issues that prompted the JSA demonstration and its evaluation, as well as a brief review of findings from the New Jersey demonstration. The chapter then describes the design parameters authorizing the JSA legislation and discusses how more recent legislation affected those parameters. Next we provide an overview of the demonstration design. The final section of the chapter explains how this report is organized.

[1]For convenience, we refer to the District of Columbia as a state.

A. POLICY CONTEXT

The JSA demonstration grew out of an effort to more effectively serve permanently laid-off workers. This section describes these workers and the various initiatives that preceded the JSA demonstration.

1. Worker Dislocation and UI

The re-employment problems of permanently laid-off workers have received national attention since the 1980s. Such workers have been called "dislocated" or "displaced." After a layoff, many face long spells of unemployment and reduced wages when they are employed again.

Since 1984, the Bureau of Labor Statistics (BLS) of the U.S. Department of Labor (DOL) has identified and tracked changes in worker dislocation through biannual supplements to the Current Population Survey (CPS). In this survey, workers who report "having lost or left a job because of a plant closing, an employer going out of business, a layoff from which they were not recalled, or other similar reason" are classified as dislocated. The 1994 survey showed that about 5.5 million workers were dislocated in 1991 to 1992. About half of this group, 2.8 million, had been employed in their jobs for three or more years (Gardner 1995). Displacements declined somewhat over the early 1990s. Data from the most recent CPS supplement demonstrate that in 1995 to 1996, 2.2 million workers were dislocated from jobs they held for three or more years (Hipple 1999).

In an analysis of these data on dislocated workers, the Congressional Budget Office (CBO 1993) found that about 2 million workers were dislocated each year during the 1980s. Although the numbers were higher than average during the recession of the early 1980s, substantial numbers were dislocated in all years, including those in which the unemployment rate was relatively low. The CBO study also found that workers in goods-producing industries--agriculture, mining, construction, and manufacturing--and in blue-collar occupations were at greater risk of dislocation than workers

in service-producing industries or white-collar occupations. However, many dislocated workers had been employed in service-producing industries and white-collar occupations as well. Moreover, differences in the risk of dislocation for these groups narrowed during the 1980s, a trend that continued in the early to mid-1990s (Gardner 1995 and Hipple 1999).

The CBO study also showed that many dislocated workers had long spells of unemployment and lower wages after re-employment. One to three years after losing their jobs, half of them were either not working or had new jobs with weekly earnings less than 80 percent of their prior earnings. The workers with the largest losses had the least education, were the oldest, and had the longest tenure with the previous employer. Furthermore, dislocated workers who held a job at the time of the survey had had relatively long jobless spells--the average duration was just under 20 weeks.

Other recent studies, which are summarized in Kletzer (1998), consistently demonstrate that a substantial earnings loss is associated with dislocation. Based on data from the Displaced Worker Surveys, Farber (1997) found that for workers dislocated between 1981 and 1995, real weekly postdislocation earnings were 13 percent lower than predislocation earnings. Jacobson, LaLonde and Sullivan (1993) used data from Pennsylvania to estimate that earnings of dislocated workers four years after separation were nearly $2,000 per quarter less than their nonseparated counterparts. Stevens (1997) also found large and persistent earnings losses due to dislocation based on a sample drawn from the Panel Study of Income Dynamics. One year after dislocation, earnings were 15 percent lower for dislocated workers than for a comparison group of nondislocated workers.

The UI system may be a convenient source for finding dislocated workers. First, many dislocated workers enter the UI system. For instance, the CBO study found that 70 percent of dislocated workers who were jobless for at least five weeks reported receiving UI benefits. More than half of these UI recipients were unemployed long enough to exhaust their benefits. More recently, in 1995

to 1996, about one-half of dislocated workers reported receiving UI benefits after their jobs loss. The lower rate of UI receipt in 1995 to 1996 probably reflects the greater availability of jobs during that period, which enabled dislocated workers to find jobs very soon after, or even immediately following, their job loss.

Another indication that the UI system is a useful tool for identifying dislocated workers is that many UI recipients can be classified as dislocated because they are permanently separated from their previous employers. Data from a study of UI recipients (Corson and Dynarski 1990) show that more than half of the UI population had no expectations of being recalled to their previous employer at the time they entered the UI system. Furthermore, about 36 percent of UI recipients in this study could be characterized as dislocated according to the BLS definition, which includes workers who lost their jobs because their plants closed, their employer went out of business, or their employer laid them off and they were not recalled.

Dislocated workers who enter the UI system, like dislocated workers in general, have longer-than-average spells of unemployment and a greater likelihood of wage reductions than other claimants. Corson and Dynarski (1990) used their sample of UI claimants from 1988 to compare employment and UI benefit outcomes of dislocated and nondislocated workers. They found that dislocated workers, particularly those with substantial job tenure, had lower re-employment rates, longer spells of unemployment, higher UI exhaustion rates, and a lower ratio of post-UI to pre-UI weekly wages than did other claimants. For example, only 81 percent of the dislocated workers with three or more years of job tenure had become re-employed during the first 20 months after their initial claim; 92 percent of the nondislocated workers had become re-employed.

These findings about re-employment problems suggest that dislocated workers may benefit from re-employment services. Moreover, since most dislocated workers who suffer long-term

unemployment enter the UI system, the UI system seems an effective mechanism to identify dislocated workers and direct services to them.

2. The New Jersey UI Re-employment Demonstration

The New Jersey UI Re-employment Demonstration was conducted in 1986 to 1987 to "examine whether the UI system could be used to identify workers early in their unemployment spells and to provide them with alternative, early intervention services to accelerate their return to work" (Corson et al. 1989). Overall, 8,675 UI claimants were assigned randomly to one of three treatments (job search assistance only, job search assistance combined with training or relocation assistance, and job search assistance combined with a cash bonus for early re-employment). The groups were compared with a randomly selected control group of 2,385 claimants who received only regular services.

Demonstration services were targeted at dislocated workers through a series of eligibility screens that excluded workers who (1) did not receive a UI first payment within five weeks after their initial claim, (2) were collecting partial UI benefits, (3) were younger than 25, (4) had fewer than three years of employment on their last job, (5) had a specific recall date from their employer, or (6) were usually hired through union hiring hall arrangements. Together these screens excluded approximately 73 percent of workers who received a first payment from UI during the sample period. Each treatment in the New Jersey demonstration had a statistically significant effect on reducing the collection of UI benefits and raising subsequent employment and earnings (Corson et al. 1989). The benefits of the treatments exceeded their total costs for society and the individuals involved. From the perspective of government alone, however, only the job search and re-employment bonus treatments were entirely beneficial. No clear evidence emerged that providing training or relocation assistance in addition to job search assistance led to cost-effective gains.

3. Legislative and Programmatic Environment

The Emergency Unemployment Compensation Act of 1991 authorized DOL to conduct the JSA demonstration to test the feasibility of implementing job search assistance programs and measure their effectiveness in promoting rapid re-employment of claimants. The legislation specified three eligibility criteria. To be eligible, UI claimants (1) had to have had job tenure at their last employer of at least 126 weeks at $30 or more per week during the three years ending on the last day of the base period, (2) had to have had no definite date for recall to former employment, and (3) could not be seeking work through a union hiring hall or similar arrangement.

The legislation specified that the demonstration should test a basic job search assistance package similar to the one tested in New Jersey and test alternative treatments building on the basic treatment. The legislation also specified that an experimental design with random assignment of eligible claimants to a treatment group or a control group not receiving demonstration services be used to measure the effectiveness of the treatments in promoting re-employment. Claimants assigned to a treatment group were required to participate in basic treatment services in order to continue to receive UI benefits.

Legislation enacted before implementation of the JSA demonstration affected this design. Specifically, the November 1993 Unemployment Compensation Amendments (Public Law 103-152) required states to implement a system to identify or "profile" claimants who are likely to exhaust regular compensation and to need re-employment services. These claimants were to be referred to services designed to help them become re-employed more quickly. Their participation in these services was to be mandatory. The legislation further directed states to implement these new Worker Profiling and Re-employment Service (WPRS) systems within a year, which meant they were in place before implementation of the demonstration.

These new requirements potentially affected the demonstration in two ways. First, the eligibility conditions specified in the legislation authorizing the JSA demonstration differed from the conditions specified for worker profiling. Therefore, it was likely that different groups of eligible individuals would be identified by the two systems. Because it did not make sense for states to implement two sets of eligibility conditions and procedures--one for the demonstration and one for profiling--congressional staff and DOL agreed to allow states to use the eligibility conditions and procedures selected for profiling for the demonstration as well. Moreover, while DOL gave states latitude in choosing eligibility conditions for profiling, both demonstration states agreed to use similar profiling models, which promoted consistency in the research design across the two states.

Second, implementation of WPRS systems could affect the service environment of the control group for the JSA demonstration and affect the impacts measured in the demonstration. If the demonstration states provided an extensive set of re-employment services to the claimants identified through profiling, it would be difficult to measure demonstration treatment impacts because one or more of the treatments would probably be similar to the services environment faced by the control group.

However, the UI claimant population in the District of Columbia (D.C.) was too small to support implementation of the demonstration and a WPRS system, and the decision was made to implement the demonstration initially and then implement a WPRS system once the demonstration was completed.[2] Since the JSA demonstration was essentially a test of the WPRS concept, the transition from the demonstration to WPRS would be straightforward. Florida made a similar decision by deciding not to implement its WPRS system in the local offices chosen for the demonstration until

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[2]Because the demonstration eligibility procedures and services were similar to those of a WPRS system and because the demonstration encompassed all offices in D.C., implementation of the demonstration in D.C. satisfied the federal requirement to implement a WPRS system.

the demonstration was completed. The demonstration was considered a temporary substitute for WPRS.

B. OVERVIEW OF THE DEMONSTRATION DESIGN

The JSA demonstration implemented three packages of re-employment services. Eligible claimants were identified early in their claim periods, using a profiling model. These claimants were then assigned to the control group or to one of three treatments: (1) structured job search assistance, (2) individualized job search assistance, or (3) individualized job search assistance with training.

D.C. and Florida were chosen for implementing the demonstration based on plans submitted to DOL. In D.C., 8,071 eligible claimants were selected from the entire new UI claimant population during a one-year intake period (June 1995 to June 1996). In Florida, 12,042 eligible claimants were selected from new claimants in 10 local Jobs and Benefits offices over a one-year intake period (March 1995 to March 1996).[3]

1. Eligibility Criteria for the Demonstration

A profiling model approach developed by DOL was used to identify claimants eligible for the demonstration. This model used a two-step process, described below, to identify the claimants expected to experience long spells of unemployment.

In the first step, claimants were screened out of the demonstration if they had characteristics that suggested the demonstration services were clearly inappropriate for them. The intent was to exclude claimants who had permanent ties to their previous employer or some other reason to not actively

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[3]Shortly before the demonstration, Florida merged UI field claims operations with Job Service offices. The new entity, Jobs and Benefits, is responsible for both UI claims and Job Service programs, and the local offices are now called Jobs and Benefits (J&B) offices. To simplify our presentation, we sometimes refer to local J&B offices as UI offices and local J&B staff as UI or Job Service staff when we are discussing D.C. and Florida together.

search for work, claimants who had already been unemployed for a long time (which would nullify the early intervention goal), and claimants who faced severe obstacles to participating in the demonstration. The specific screens used in each state are discussed in Chapter III.

Then, state-specific regression models were used to predict the probability of exhaustion of benefits, and claimants with a high probability of exhaustion were chosen as the demonstration-eligible population. These regression models, which were based on a national model developed by DOL, used five variables to predict exhaustion: (1) the claimant's pre-UI industry, (2) the claimant's pre-UI occupation, (3) the claimant's education, (4) the claimant's years of tenure on the pre-UI job, and (5) the local unemployment rate. These models are described in greater detail in Appendix B.

2. Re-employment Service Packages

Eligible claimants were identified as soon as the first UI payment was made. They were then randomly assigned to one of three treatment groups that received demonstration services or a control group that received regular services. The three treatment groups, as shown in Figure I.1, were offered the following services:

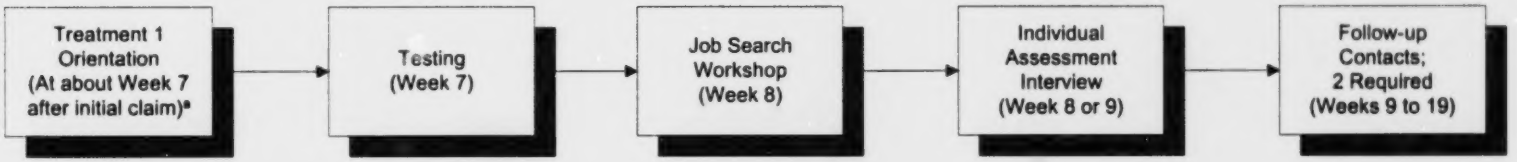
1. ***Structured Job Search Assistance (SJSA).*** This treatment replicated the basic job search assistance treatment tested in New Jersey. Claimants were sent a letter during the fourth week of unemployment telling them to report to a Job Service orientation session.[4] Claimants reported for orientation two to three weeks later, approximately during the sixth or seventh week of unemployment.

[4]This description of the timing of services assumes claimants applied for UI benefits as soon as they were laid off.

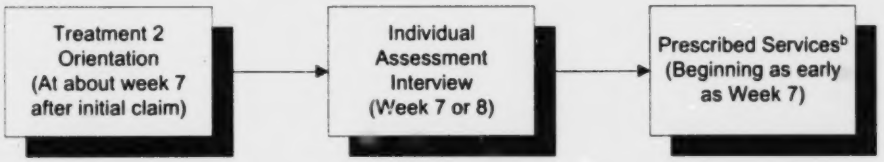
FIGURE I.1

DESIGN OF TREATMENTS, BEGINNING WITH ORIENTATION

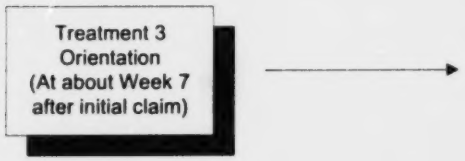
Treatment 1: Structured Job Search Assistance


Treatment 1 Orientation (At about Week 7 after initial claim)[a]
Testing (Week 7)
Job Search Workshop (Week 8)
Individual Assessment Interview (Week 8 or 9)
Follow-up Contacts; 2 Required (Weeks 9 to 19)

Treatment 2: Individualized Job Search Assistance


Treatment 2 Orientation (At about week 7 after initial claim)
Individual Assessment Interview (Week 7 or 8)
Prescribed Services[b] (Beginning as early as Week 7)

Treatment 3: Individualized Job Search Assistance with Training



The rest of Treatment 3 was the same as for Treatment 2, but demonstration staff were expected to discuss EDWAA services during the individual assessment interview, and, if deemed appropriate, the claimant was to meet with an EDWAA representative as part of the prescribed services

[a]The timing of orientation was expected to vary because of different timing of initial UI procedures in the two states or because of the cycle of service delivery (see discussion of service timing in Chapters IV and V).

[b]Prescribed services could include testing, job search workshop, counseling, and follow-up contracts (may require more or fewer contacts than Treatment 1). Participants also could be referred to other services for which participation was not to be monitored (for example, job clubs or use of the computerized job listings and the resource center).

At the orientation, claimants were told about the re-employment services available to them and specifically about demonstration services. They were tested the same week and scheduled for an approximately 15-hour job search workshop the following week. After the workshop, they were scheduled for a one-on-one assessment/counseling interview to discuss their re-employment plans. Attendance at this initial set of services was mandatory, unless the claimant was explicitly excused from services. Claimants who failed to participate could lose some of their UI benefits. In subsequent chapters, we describe the participation rates in each of the services and the enforcement of the participation requirements.

Individuals who continued collecting UI benefits were to have a minimum of two additional contacts with staff. These contacts could take any form. In addition, each office established a job search resource center for demonstration participants, and participants could receive any other services offered by the Job Service. However, these services were not mandatory.

2. ***Individualized Job Search Assistance (IJSA).*** This treatment was similar to the job search assistance treatment, except that a decision was made on an individual basis about the services a claimant should receive. Eligible claimants were sent a letter telling them to report for a group orientation session held during approximately their sixth or seventh week of unemployment. At that session, they were given an overview of the services available to them and scheduled for an individual assessment interview later that week or the next week. An individual service plan was developed during the assessment interview. This plan varied across individuals, but the specified services--such as testing, the job search workshop, or additional assessment/counseling interviews--were mandatory. Additional ongoing contacts could also be required. Claimants in this treatment could also receive any other services, such as placement assistance, from the Job Service, but these services were not mandatory. In subsequent chapters we describe the service participation and the use of the service plan for the typical claimant assigned to this treatment.

3. ***Individualized Job Search Assistance with Training (IJSA+).*** This treatment was identical to the second treatment, but included a special effort to enroll interested claimants in training. In all treatments, EDWAA training was described during the orientation session. Claimants who expressed interest in training were referred to EDWAA. However, in this treatment, the discussion of training opportunities during orientation was more extensive and, if possible, made by an EDWAA staff member.

   In addition, the possibility of training was explicitly discussed during the individual assessment interview. Any claimant who expressed interest was scheduled to talk to an EDWAA staff member. If possible, this discussion took place in the Job Service office immediately following the assessment interview.

This "one-stop" approach to service delivery was to be facilitated by having assessment interviews on days when an EDWAA staff member could be at the Job Service office. To ensure that training was available, the states participating in the demonstration were asked to designate a portion of their EDWAA funds to provide training to members of this group.

C. RESEARCH METHODOLOGY AND DATA

A central feature of the JSA demonstration is the random assignment of demonstration-eligible UI claimants to a control group or one of the three treatment groups. Random assignment ensures that the treatment and control groups exhibit similar characteristics and each is representative of the target population. Based on this random assignment design, any differences in outcomes between each of the three treatment groups and the control group are directly attributable to the JSA service packages and the differences therefore provide unbiased estimates of the treatment net impacts. Given this, we estimate the net impacts of the three treatments separately by comparing the average outcome for each treatment group with the average outcome for the control group.[5] In a couple of cases we extend the research methodology beyond the simple treatment-control differences in order to address specific research questions that are beyond the scope of the treatment-control design. The cases are highlighted in the text of the report, where we also describe the methodology that is used.

The claimant-level data used in the impact analysis come from four sources: the JSA demonstration participant tracking system (PTS), UI program data, UI wage records, and a one-year follow-up survey of a subset of demonstration claimants. We also use data from site visits and published reports to describe the demonstration environments (Chapter II), estimate the program

[5]We also tested whether using regression models to adjust for claimant characteristics would generate different impact estimates and found that the adjusted estimates were nearly identical to the unadjusted treatment-control differences. We decided therefore to report only the unadjusted treatment-control differences.

costs (Chapter IV), and provide other contextual data to interpret the findings from the analysis of claimant-level data.

1. JSA Demonstration PTS

We describe the participation in and timing of JSA services (Chapter III) based on data drawn from the JSA demonstration PTS. The PTS was a computer-based system that was used to assist in the operation and monitoring of the demonstration. It was also used to identify and select the sample, to assign eligible claimants randomly to the treatment and control groups, and to send notices to claimants to report to services. With respect to services, the PTS contained data on service assignment, attendance, and timing.

2. UI Program Data

We estimate the impacts of the JSA treatments on UI outcomes (Chapters V and VI) based on UI program data collected from each state. At the end of the JSA demonstration operations, we received special data extracts from the UI mainframe system in each state. These extracts contained UI payment records that were used to create measures of UI benefit receipt for each claimant in the demonstration. These extracts also contained data on the UI base period earnings, the pre-UI employer, and UI determinations and denials.

3. UI Wage Records

Our estimates of the impacts of the JSA treatments on employment and earnings (Chapter VII) are based primarily on quarterly earnings data drawn from the state UI wage records. The advantage of this source of earnings data is that wage records can be easily obtained for most sample members. However, the wage records provide somewhat crude measure of post-UI earnings. Because the data are organized by quarter, they cannot be used to determine the point at which claimants become

reemployed after claiming UI. Furthermore, the wage records do not include earnings from self-employment, federal jobs, military services, or domestic or agricultural employment.

Given these limitations, we collected additional detail on employment and earnings as part of the follow-up survey. But because of the broader coverage provided by UI wage records and the potential nonresponse bias associated with the follow-up survey data (see Appendix A), we use the wage records as our primary source of data on employment and earnings outcomes.

4. Follow-Up Survey Data

The follow-up survey was conducted approximately one year after enrollment. The survey provided data on the following:

- Demographic information, such as household composition and education
- Characteristics of the prelayoff job
- Perceptions of and attitudes towards the JSA demonstration services
- Participation in alternative job-search programs
- Postlayoff job search behavior, employment, and earnings
- The length of postlayoff unemployment
- Sources and amounts of non-UI income, including the labor force activities of spouses during the pre- and postlayoff periods

Although we collected information on employment and earnings from the UI wage records, we also collected these data from the follow-up survey. The survey data on employment and earnings are useful both to potentially address some of the limitations of the wage records data and to provide detailed information on the characteristics of post-UI jobs. For this report, survey data were also used to construct the outcomes measures for the analyses of customer satisfaction with JSA services

(Chapter III) and the treatment impacts on job search activities and post-UI job characteristics (Chapters VIII and IX). The details associated with the follow up survey are described in Appendix A.

D. CONTENT OF THE REPORT

This report on the JSA demonstration examines the operation and impacts of the demonstration in D.C. and Florida. We first describe the economic and service environments in D.C. and Florida, as well as the UI population targeted by the demonstration in each state. These issues are addressed in Chapter II. The next two chapters address the operation of the demonstration--Chapter III describes the services received by claimants and the timing of the services, while Chapter IV presents the estimated costs of providing the services. Chapters V, VI, VII, VIII, and IX present our estimates of the impact of the demonstration on various claimant outcomes--UI receipt, UI benefit eligibility, employment and earnings, job characteristics, and job search activities. Chapter X presents estimates of the cost-effectiveness of the treatments. Chapter XI summarizes our findings and discusses the implications of our findings for the new WPRS systems and for displaced worker policy in general.

II. THE DEMONSTRATION ENVIRONMENT AND ELIGIBLE POPULATION

Economic conditions influence whether individuals can obtain employment and their decisions about when to accept employment. Understanding the characteristics of the eligible population and the prevailing economic conditions will help us assess whether the demonstration findings can be generalized for claimant populations outside the demonstration. In this chapter, we review the process used to select local sites for the demonstration and describe the economic environments in these sites and in D.C. and Florida more broadly during the demonstration. We also describe state and local factors related directly to the demonstration. First, we consider the characteristics of the claimants who were determined to be eligible for the demonstration in each state and site. Second, we describe the organization and staffing of local offices and agencies participating in the demonstration. Finally, we examine the approach each state took to monitor and enforce compliance with the participation requirements.

General economic conditions in both states at the beginning of the demonstrations were somewhat less favorable than in the nation as a whole. However, both states had recent histories of high economic growth, and both, especially D.C., depend primarily on non-manufacturing industries with relatively high growth in recent years.

The claimant population targeted by the demonstrations was somewhat older in Florida than in D.C. Florida also had both more high school dropouts and college graduates among its claimants than D.C. did. The Florida claimants were much more likely than the D.C. claimants to have held manufacturing jobs, while D.C. claimants were more likely to have held service or public administration jobs. In Florida, the characteristics of the eligible claimants varied greatly across sites. Compared with claimants nationwide, the D.C. and Florida demonstrations tended to include more women claimants, fewer young claimants, more African American and Hispanic claimants, and many fewer claimants from manufacturing.

A. SELECTION OF LOCAL OFFICES

Our selection of local offices in both demonstration states was designed to maximize the generalizability of the demonstration findings to the entire state. In D.C., achieving generalizability to the entire state was relatively straightforward because of its small size. The demonstration served claimants from every UI office and those applying in suburban offices for benefits from D.C.[1] The demonstration participants were selected randomly from those claimants who were determined to be eligible based on the profiling and selection procedures that we describe in Appendix B.

In Florida, we did not include every local Jobs and Benefits (J&B) office in the demonstration because providing services in every office in the state would have entailed large operational burden and costs. A more cost-effective approach was to select a random sample of local offices and then select a random sample of claimants from each of the selected offices. Selecting offices and claimants randomly ensures that the demonstration-eligible claimants are representative of the Florida UI population and that the results of the demonstration can be applied to the state population.

A related design objective was to assign each potential eligible claimant an equal probability of selection. This procedure yields the most efficient sample design for generalizing to the entire eligible population if no particular subgroup of the eligible population is of greater interest than another. In D.C., where the entire UI population was covered by the demonstration, we selected claimants at random from the eligible UI population without regard for where they filed their initial UI claim. In Florida, the objective of equal probability of

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[1]Because Washington, D.C. is part of a large metropolitan area, it allows claimants filing for benefits to file their initial claim in one of the suburban Virginia or Maryland offices. The D.C. UI system treats these claimants the same as if they had filed their claim in a D.C. office, so we included them in the demonstration.

selection implied that local offices should be selected with the probability of selection proportional to the size of the eligible population and that equal numbers of eligible claimants be offered services in each office.

A final objective in selecting local offices in Florida was to achieve a broad representation of local offices and a broad representation of eligible claimants. To help achieve this objective, the local offices were stratified geographically and selected randomly within the geographical strata.

Two other issues were taken into account in selecting Florida offices. First, some offices served a population not suitable for the demonstration. A few offices served primarily agricultural workers on seasonal layoffs, so we excluded these because most claimants would not be looking for a new job. The state identified three of the 49 local offices according to this criterion. Second, some offices were considered too small to provide an adequate sample of eligible claimants for the demonstration. We excluded 16 additional offices that had average claimant populations below 3,200 in 1992 and 1993. We also excluded the Tampa office because it was experimenting with local service initiatives, including one-stop shopping, and it therefore did not provide an accurate representation of typical Florida local J&B operations. In addition, we wanted to avoid the potential for confusing local staff by asking them to implement another service initiative. Although the number of offices excluded in Florida (20) was high, the excluded offices represented only 16 percent of the claimant population.

We selected 10 local Florida offices based on this process: Pensacola, St. Augustine, Orlando, Lakeland, Clearwater, Fort Pierce, Fort Lauderdale, Davie, Hialeah, and Miami (see Figure II.1).

B. CHARACTERISTICS OF DEMONSTRATION SITES

The two areas operating the JSA demonstration are obviously quite different. Florida is one of the most populous states in the United States and has a recent history of extraordinarily high economic and population growth. The state encompasses several large cities, as well as many smaller cities and

FIGURE II.1

FLORIDA LOCAL OFFICES PARTICIPATING IN THE JSA DEMONSTRATION

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large rural areas. In contrast, Washington, D.C., is a single city, and the federal government is the largest employer. Although metropolitan Washington has grown rapidly in recent decades, the city itself has lost population. Both D.C. and Florida had somewhat mixed economic conditions during the period of the demonstration, as we describe in this section.

1. District of Columbia

The city of Washington D.C. has a population of 570,000 residents; the Washington Metropolitan Statistical Area (MSA), includes nearly 4.5 million residents from the city and its surrounding suburbs. Both Washington, D.C., and the Washington MSA provide useful information about the economy and labor markets UI claimants face. Many of the claimants either already live outside D.C. or have easy access to employers located in the suburbs, so it is sensible to use the metropolitan area as representative of the local economy. Other claimants may have more limited access to the Washington suburbs, so D.C. may provide a more relevant definition of the local market for those claimants.

The economic statistics suggest that economic conditions were relatively favorable in the Washington metropolitan area, but not as favorable within D.C. For example, the metropolitan area had an unemployment rate in 1994 of only 4 percent, which was considerably lower than the national rate of 6 percent (Table II.1). In contrast, D.C. had an unemployment rate of 8.2 percent. Similarly, between 1990 and 1994 employment grew by 5.9 percent in metropolitan Washington, compared with a 6.1 percent decline in D.C. Metropolitan Washington has little employment in manufacturing--only 2 percent within D.C. and 4 percent in the metropolitan area. Not surprisingly, the proportion of government employment is high. However, during the demonstration, government employment was declining. For D.C., the decline in government employment translated into an overall employment decline during this period. For the overall metropolitan area, employment growth

TABLE II.1

CHARACTERISTICS OF THE D.C. DEMONSTRATION SITE

	District of Columbia	Washington Primary MSA
Population (1994, in thousands)	570	4,474
Population Growth (1990-94, percent)	-6.1	5.9
Unemployment Rate (1994, percent)	8.2	4.0
Percent Manufacturing Employment (1995)	2.1	4.0
Percent Government Employment (1995)	38.6	25.3
Percent Change in Employment (October 1994 to October 1995)		
Total	-2.6	0.5
Manufacturing	0.8	2.5
Trade	0.2	2.0
Services	-0.6	2.7
Government	-6.1	-2.9
Per Capita Income (1993, dollars)	29,500	25,956[a]

[a]Based on the Washington-Baltimore consolidated MSA.

MSA = Metropolitan statistical area.

SOURCE: District of Columbia Department of Employment Services, *Area Labor Summary*, December 1995; U.S. Department of Commerce, Bureau of the Census, *Statistical Abstract of the United States, 1995.*

in other industries compensated for the decline in government employment, so employment overall increased slightly.

2. Florida

Although Florida has enjoyed many years of rapid economic growth, the unemployment rate for Florida (6.6 percent) was higher than for the nation (6.1 percent) in 1994, when the demonstration began. For the period from April 1995 to April 1996, when most demonstration participants were searching for work, total employment in Florida grew by 3.2 percent. Strong state growth in service employment compensated for a slight decline in manufacturing employment over this period (Table II.2).

In selecting local offices in Florida for the demonstration, the offices were stratified geographically and then selected randomly from the strata with probability of selection proportional to size. The offices selected for the demonstration therefore were scattered throughout the state (see Figure II.1), and many of the offices were located in relatively large population centers.

Four of the 10 demonstration offices in Florida were located in the populous southeastern coastal area of the state near the city of Miami (see Figure II.1). Two offices, Fort Lauderdale and Davie, were located in Broward County; two other offices, Miami and Hialeah, were located in neighboring Dade County. The populations of Broward and Dade counties, which are the largest in Florida, are concentrated in urban/suburban communities along the coast of the Atlantic Ocean. Population and employment growth have been relatively strong in Broward County in recent years. The 1994 unemployment rate in Broward County was nearly 6.5 percent, which was comparable to Florida as a whole (Table II.2). Between April 1995 and April 1996, which covers most of the demonstration period, total employment in the Fort Lauderdale MSA grew by nearly 4.5 percent. Employment

TABLE II.2

CHARACTERISTICS OF THE FLORIDA DEMONSTRATION SITES

Characteristic	County (Local Offices in Parentheses)									
	Broward (Ft. Lauderdale, Davie)	Dade (Miami, Hialeah)	Escambia (Pensacola)	Orange (Orlando)	Pinellas (Clearwater)	Polk (Lakeland)	St. Johns (St. Augustine)	St. Lucie (Ft. Pierce)	All Sites[a]	Statewide
Population (1994, in thousands)	1,340	1,990	277	740	871	437	95	167	5,917	12,937
Population Rank	2	1	15	6	5	8	32	22	---	---
Population/Square Mile (1994)	1,109	1,023	417	815	3,110	233	156	291	715	257
Population Growth (1990-1994, percent)	6.75	2.75	5.43	9.25	2.24	7.85	13.04	11.08	7.29	7.27
Unemployment Rate (1994, percent)	6.5	8.0	5.1	5.8	5.2	8.3	5.9	13.4	7.3	6.6
Percent Manufacturing Employment (1994)	6.4	7.8	6.6	6.7	10.2	10.4	8.2	4.5	7.6	7.3
Percent Change in Manufacturing Employment (1983 to 1993)	1.0	-9.8	-11.5	20.2	9.9	-5.7	68.0	34.6	13.3	6.0

TABLE II.2 (*continued*)

Characteristic	County (Local Offices in Parentheses)									
	Broward (Ft. Lauderdale, Davie)	Dade (Miami, Hialeah)	Escambia (Pensacola)	Orange (Orlando)	Pinellas (Clearwater)	Polk (Lakeland)	St. Johns (St. Augustine)	St. Lucie (Ft. Pierce)	All Sites[a]	Statewide
Percent Change in MSA Employment (April 1995 to April 1996)										
Total	4.5	2.8	3.2	3.2	3.1	2.0	2.5	2.4	3.0	3.2
Manufacturing	2.0	-2.9	0.0	-1.6	-0.8	-2.8	0.9	-7.7	-1.6	-0.8
Trade	5.0	2.0	2.6	2.9	3.3	4.7	3.0	3.9	3.4	3.1
Services	5.6	5.2	7.0	5.1	4.3	2.5	5.2	2.2	4.6	4.7
Construction	3.7	4.1	4.3	4.7	2.3	1.3	3.6	1.9	3.2	3.3
Per Capita Income (1993, dollars)	23,840	19,266	16,899	19,607	22,798	16,858	24,797	15,773	19,980	20,650
Rank	7	16	27	15	9	28	6	34	---	---

SOURCE: Florida Bureau of Labor Market Information, file libraries, Worldwide Website; Florida Department of Commerce, county profiles and county comparisons, Worldwide Website.

[a]Simple average of the county numbers; not adjusted for size of counties.

[b]Refers to the Metropolitan Statistical Area (MSA) in which the demonstration office is located or the nearest MSA.

growth was especially strong in trade and services employment, which grew by more than 5 percent. The 1993 per capita income in Broward County was $23,840, which was higher than for the state as a whole and ranked seventh among all Florida counties.

Dade County includes the city of Miami and the corresponding suburban areas. Population and employment growth in Dade County have been slower than growth in Broward in recent years. Between 1990 and 1994, the population of Dade County grew by less than 3 percent. The unemployment rate for the county was 8.0 percent, somewhat higher than the statewide rate of 6.6 percent. The proportion of county employment in manufacturing is relatively modest (7.8 percent), and this proportion declined between 1983 and 1993, as manufacturing employment declined by nearly 10 percent. Manufacturing employment continued to decline, at least in the Miami MSA, by 3 percent between April 1995 and April 1996. Employment growth over this period in Miami was strongest in services and construction. The per capita income in Dade County in 1993 was $19,266, which ranked 16th among the counties but was less than the statewide income figure ($20,650).

Two other demonstration sites were located in communities on the Atlantic Coast. St. Augustine and Fort Pierce are located in the least populous counties with demonstration offices, St. Johns County and St. Lucie County, respectively. Although the populations in these counties are small, they grew rapidly between 1990 and 1994--by 13 percent in St. Johns and 11 percent in St. Lucie. The economic conditions in these two counties are very different. St. Johns is a relatively affluent county with a per capita income in 1993 of $24,797--sixth among all counties in the state and first among the counties with demonstration sites. The 1994 unemployment rate in St. Johns was 5.9 percent, which was below the average for the state and for the demonstration sites. Manufacturing employment, although a modest proportion (8.2 percent) of all employment, grew rapidly between 1983 and 1993. More recently, employment growth in St. Augustine was 2.4 percent between April 1995 and April 1996, which was below the state average. The highest employment growth occurred in services.

Compared with St. Johns County, St. Lucie is less affluent and has encountered less favorable economic conditions and slower employment growth. St. Lucie's per capita income in 1993 was $15,773, substantially below the state average and the lowest income level among the counties with demonstration sites. The 1994 unemployment rate (13.4 percent) was about twice the state rate and the highest rate among the demonstration sites. This high unemployment rate may be an indication that job seekers in St. Lucie County faced substantial barriers to re-employment during the time of the demonstration. Despite the high county unemployment rate in 1994, the rate of employment growth in Fort Pierce between April 1995 and April 1996 was 2.4 percent, which was similar to the rate for St. Johns County and below the state rate.

Three of the demonstration offices (Clearwater, Lakeland, and Orlando) are located in central Florida. Clearwater and Lakeland are located in Pinellas County and Polk County, respectively, which have had relatively slow population growth in recent years. Population in both of these counties grew by less than 3 percent between 1990 and 1994, which is less than half the rate for the state. Total employment in the Clearwater and Lakeland MSAs expanded during the demonstration as strong growth in services and trade made up for the decline in manufacturing employment in each site. Despite these similar trends, the 1994 unemployment rate in Polk County was a relatively high 8.3 percent, compared with a relatively low 5.2 percent in Pinellas County. This difference in economic conditions is also reflected in the income levels of the two counties. Per capita income in Pinellas County in 1993 ($22,798) was higher than the state as a whole, while that in Polk County ($16,858) was lower.

The other central Florida demonstration office, Orlando, is located in a county with strong population growth in recent years. The population of Orange County grew by 9.25 percent between 1990 and 1994. The county unemployment rate (5.8 percent) was a bit lower than that for the state as a whole. As in other Florida counties, employment in manufacturing represents a small proportion (6.7 percent) of all employment, but manufacturing employment in Orange County expanded by 20 percent between 1983 and 1993. Over the demonstration period, employment in Orlando grew by slightly more than 3 percent, matching the growth rate

statewide. Orange County is not one of the wealthiest counties in the demonstration--it ranks 15th among Florida counties, with per capita income that was slightly lower in 1993 than that of the state as a whole.

The remaining demonstration site, Pensacola, is the largest city in Escambia County in the Florida panhandle. In 1994, Escambia had an unemployment rate of 5.1 percent, which was the lowest of the demonstration sites. Only 6.6 percent of the employed residents in 1994 held jobs in manufacturing. Government employment represents a large proportion of the county workforce--second only to services in 1994. The large government employment arises because of the location of large military bases in the Pensacola area. Manufacturing employment in Pensacola remained constant over the demonstration period, while service and construction employment expanded rapidly. The overall employment growth rate was comparable to the state as a whole. The income level in Escambia is relatively low--$16,899 in 1994, considerably lower than the statewide average.

C. CHARACTERISTICS OF THE ELIGIBLE POPULATION

Eligibility of the claimants for the demonstration was based on data collected from claimants when they filed their initial UI claim. We make comparisons between the eligible claimants in the two states, and we compare the claimants in the two states with a nationwide sample of UI claimants from 1988 (Corson and Dynarski 1990). The measures we present for the eligible population are based on the demonstration participants--those claimants assigned to one of the treatment groups or the control group. Since this group is a random sample of all eligible claimants, measures based on this group provide valid estimates of the corresponding measures for the eligible population.

Differences between the eligible claimants in the demonstration states and the nationwide sample arise because of three factors. First, there are differences between claimants in D.C. and Florida and claimants nationwide. For example, relatively few claimants in D.C. or Florida come from manufacturing because of the limited manufacturing employment in either state. Second, the demonstration eligibility criteria also affect

the degree to which the eligible claimants in the demonstrations differ from claimants nationwide. For example, the eligibility criteria screen out claimants who have an expected date of recall to their previous employer. Since recall is more prevalent in manufacturing than in other industries, the eligibility criteria contribute to D.C. and Florida demonstration eligible claimants being less likely to be from manufacturing than claimants nationwide. Finally, there may be differences between new UI claimants in 1988 and new claimants in 1995 and 1996. This factor probably plays a minor role compared with the other two factors in explaining the differences between the claimant samples.

1. By State

Eligible claimants in the D.C. and Florida demonstrations had very different characteristics. As shown in Table II.3, the representation of women in the eligible populations in both states was higher than the representation of women in the nationally representative sample of all UI claimants from 1988.[2] According to the 1988 sample, about 41 percent of all U.S. claimants were women, as shown in the final column of Table II.3, compared with 46 percent of eligible claimants in Florida and 55 percent of eligible claimants in D.C. This difference probably arises from the initial eligibility screens used in the JSA selection process. Some of the screens, such as the union hiring hall screen, may

[2]The statistics presented in Table II.3 differ slightly from the corresponding statistics on sample characteristics presented in the JSA implementation report (Decker, Perez-Johnson, and Corson). The differences arise because in this report we have excluded claimants who entered the demonstration during the pilot phase. These claimants were included in the implementation report.

TABLE II.3

CHARACTERISTICS OF THE DEMONSTRATION ELIGIBLES
(Percent)

	Florida Local Office												
	Pensacola	St. Augustine	Clearwater	Orlando	Lakeland	Ft. Pierce	Ft. Lauderdale	Davie	Hialeah	Miami	All Florida Sites	District of Columbia	U.S. UI Claimants 1988
Sex													
Male	56.8	55.3	51.5	56.3	54.9	54.3	50.0	49.6	50.5	61.3	54.0	45.5	58.9
Female	43.2	44.7	48.5	43.7	45.1	45.7	50.0	50.4	49.5	38.7	46.0	54.5	41.1
Age													
Under 25 Years	8.9	8.4	4.3	6.5	6.7	9.2	4.4	4.9	5.8	4.5	6.3	9.5	12.2
25 to 34 Years	30.5	26.8	19.1	26.8	25.8	24.0	19.3	15.5	16.3	18.2	22.0	32.6	33.4
35 to 44 Years	31.2	29.6	28.9	29.1	32.5	32.1	24.0	25.1	23.2	29.6	28.4	30.3	25.1
45 to 54 Years	19.3	19.2	26.5	21.7	20.5	19.9	27.0	27.8	24.0	23.2	23.1	18.9	17.4
55 to 64 Years	9.2	11.9	15.1	12.0	12.7	10.1	15.5	17.3	23.1	17.4	14.6	7.9	10.4
65 Years or Older	0.9	4.1	6.1	3.9	1.8	4.7	9.8	9.4	7.6	7.1	5.6	0.8	1.6
Mean (Years)	38.6	40.7	44.3	41.2	40.8	40.8	45.7	46.2	46.2	44.7	43.1	38.0	38.1
Race/Ethnicity													
White	72.4	89.4	92.4	66.2	76.8	68.5	76.9	63.8	7.1	16.7	61.5	10.4	74.8
Black	24.1	8.4	3.1	13.1	19.2	25.7	12.5	14.2	8.5	24.7	15.6	82.7	12.0
Hispanic	1.6	1.7	3.8	18.7	3.0	5.2	9.3	19.9	83.4	58.1	21.7	5.3	9.5
Other	2.0	0.5	0.8	2.0	1.0	0.6	1.4	2.1	1.0	0.5	1.2	1.6	3.7
Education[a]													
No High School Diploma	14.2	17.0	17.3	15.0	24.2	36.8	18.6	24.6	40.7	46.2	25.9	15.2	21.4
High School Diploma Only	68.0	63.3	57.0	59.8	60.6	52.8	56.3	54.8	49.8	37.1	55.6	55.0	54.6
Associate's Degree	10.1	11.4	12.8	11.6	9.4	6.9	9.9	9.0	5.2	8.5	9.4	23.5	13.5
Bachelor's Degree	7.2	7.8	12.0	12.8	5.4	3.4	14.4	10.6	3.7	7.7	8.5	4.2	8.5
Graduate School	0.5	0.6	0.9	0.7	0.4	0.1	0.9	0.9	0.5	0.5	0.6	2.2	2.0
Industry of Previous Job													
Agriculture and Mining	0.9	1.4	1.6	1.3	4.1	10.2	0.5	0.5	0.4	0.6	2.2	0.2	5.2
Construction	6.6	6.1	3.6	3.6	6.9	8.0	4.3	3.3	4.5	6.2	5.3	5.2	16.1
Manufacturing	7.4	15.7	15.8	14.5	23.8	11.1	11.7	14.7	27.6	12.0	15.7	1.4	39.5
Transportation and Utilities	5.1	6.7	6.0	8.7	5.4	7.6	7.4	9.2	9.3	7.7	7.4	3.1	4.5
Wholesale and Retail Trade	21.5	18.9	21.3	16.8	19.5	19.3	21.6	21.7	18.6	24.6	20.4	15.7	12.8
Finance, Insurance, & Real Estate	7.3	16.6	17.1	14.8	6.3	10.3	19.3	15.6	11.7	10.6	13.0	8.8	3.9
Services	36.2	30.8	32.3	35.4	31.5	30.4	33.4	32.6	26.2	35.7	32.4	56.8	15.2
Public Administration	15.0	3.7	2.1	4.9	2.3	3.2	1.8	2.4	1.7	2.6	3.7	8.8	2.9

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TABLE II.3 *(continued)*

	Florida Local Office												
	Pensacola	St. Augustine	Clearwater	Orlando	Lakeland	Ft. Pierce	Ft. Lauderdale	Davie	Hialeah	Miami	All Florida Sites	District of Columbia	U.S. UI Claimants 1988
Occupation at Previous Job													
Technical and Managerial	21.7	25.6	27.7	22.6	17.8	15.7	28.4	25.0	15.1	14.1	21.0	20.6	NA
Clerical and Sales	45.5	46.4	48.8	49.9	36.9	43.1	50.7	52.2	37.1	49.2	45.9	59.1	NA
Service	0.9	1.9	0.8	0.9	2.2	4.2	0.4	0.5	0.2	0.8	1.2	1.0	NA
Agriculture	0.6	0.9	0.7	0.7	4.0	2.7	0.7	1.0	3.0	2.3	1.7	0.1	NA
Processing	3.9	4.4	3.2	3.5	6.1	4.1	3.2	3.3	8.4	4.7	4.5	1.3	NA
Machinist Trades	1.6	1.3	4.2	2.5	6.5	2.0	3.3	4.7	16.7	5.4	5.1	0.9	NA
Benchwork	17.9	14.0	7.8	8.5	12.0	13.3	7.7	8.0	9.8	14.6	11.2	10.5	NA
Structural	7.8	5.4	6.8	11.5	14.6	14.9	5.5	5.3	9.7	8.9	9.3	6.3	NA
Tenure													
Less Than 1 Year	36.2	30.3	31.5	33.4	41.3	43.1	32.8	34.2	34.2	43.6	36.3	33.1	28.2
1 to 3 Years	22.7	25.0	25.5	27.0	23.1	22.5	25.5	23.2	28.8	25.9	25.0	28.8	24.9
3 to 10 Years	29.7	35.0	32.2	29.9	25.8	26.5	31.5	31.8	26.2	21.9	28.8	26.5	46.9[b]
10 Years or More	11.4	9.7	10.7	9.8	9.9	7.9	10.1	10.9	10.7	8.6	10.0	11.6	---[b]
Mean (Years)	4.2	4.1	4.2	3.8	3.8	3.4	3.9	4.1	3.8	3.3	3.8	4.2	5.7
UI Weekly Benefit Amount ($)	173	178	182	177	176	163	187	185	163	162	174	213	150
UI Entitlement Amount ($)	4,160	4,130	4,345	4,153	4,149	3,646	4,335	4,285	3,684	3,589	4,042	5,476	3,601
Average Base Period Earnings ($)	17,847	19,596	21,717	19,884	18,753	16,058	21,487	21,017	16,129	15,711	18,780	18,206	13,633
Estimated Probability of Benefit Exhaustion	50.1%	52.2%	51.4%	51.2%	51.7%	51.4%	51.1%	51.6%	52.3%	50.7%	51.3%	71.1%	NA
Sample Size	1,158	801	1,139	1,301	1,296	1,211	1,292	1,269	1,286	1,289	12,042	8,071	2,929

SOURCE: JSA demonstration participant tracking system.

[a]In D.C., education is coded by years completed. For this table, we categorized D.C. claimants as follows: no high school diploma (1-11 years), high school diploma only (12 years), associates degree (13-15 years), bachelor's degree (16 years), graduate school (more than 16 years)

[b]Data on job tenure for U.S. claimants are not broken down as shown. The percentage 46.9 is the proportion of U.S. claimants with job tenure of three years or more.

NA = not available

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mostly affect claimants in industries that are dominated by male workers. The screens would therefore tend to exclude male claimants from the demonstration at a higher rate than female claimants.

The Florida claimants were older than D.C. claimants by an average of five years--43.1 years old in Florida compared with 38.0 years old in D.C. Florida had many more claimants in the oldest group. Over 5 percent of the Florida claimants were 65 years or older, compared with less than 1 percent of D.C. claimants. The Florida claimants also tended to be older than the average UI claimant across the country. According to data from 1988, the average UI claimant nationwide was about 38 years old, considerably younger than the Florida claimants, but comparable to the D.C. claimants. The findings from the initial states to implement Worker Profiling and Re-employment Services systems show that similar profiling and screening methods used in those states also targeted claimants who were older on average than nontargeted claimants (Dickinson, Kreutzer, and Decker 1997).

Racial minorities made up a substantial proportion of demonstration-eligible claimants in both D.C. and Florida. A large majority of D.C. claimants (83 percent) were African American. In Florida, the majority (62 percent) of claimants were white and non-Hispanic, but a substantial proportion of claimants were African American (16 percent) or Hispanic (22 percent). In contrast, the percentages of African American and Hispanic claimants in the 1988 national sample were only 12 and 10 percent, respectively.

Demonstration-eligible claimants in the two states were distributed very differently among education levels, although our comparisons are limited by the different education codes used in the two states (see footnotes in Table II.3). Florida had more claimants than D.C. at the ends of the education scale. The proportion of claimants who were high school dropouts was considerably higher in Florida (26 percent) than in D.C. (15 percent). But the rate of college completers was also higher in Florida, by about 50 percent, than in D.C. Compared with the nationwide claimants, Florida had a higher proportion of school dropouts and a similar proportion of college completers, while D.C. claimants had a lower proportion of dropouts and a lower proportion of college completers.

Demonstration-eligible claimants in the two states also had different pre-UI job experiences. Florida claimants were about 10 times more likely than D.C. claimants to have previously held a manufacturing job--less than 2 percent of D.C. claimants held jobs in manufacturing, compared with 15.7 percent in Florida. But both states had a relatively low proportion of eligible claimants from manufacturing when compared with the 1988 national sample of UI claimants, where manufacturing workers made up nearly 40 percent of all claimants nationwide. As stated in the introduction to this section, this difference is attributable both to special characteristics of D.C. and Florida claimants in general and to the demonstration eligibility criteria. Other differences in industry between eligible claimants in D.C. and Florida were less stark than the manufacturing differences, but they were still important. D.C. claimants were less likely than Florida claimants to have had jobs in transportation and utilities, wholesale and retail trade, and finance, insurance, and real estate. On the other hand, D.C. claimants were more likely to have held jobs in services or public administration.

Average UI entitlements and weekly benefit amounts were higher among D.C. demonstration-eligible claimants than their Florida counterparts. This was at least partly because the D.C. maximum weekly benefit amount ($347 until January 1996, when it increased to $359) was considerably higher than the Florida maximum weekly benefit amount ($250). D.C. claimants had higher entitlements than Florida claimants despite having lower average base period earnings.

Finally, for each claimant we calculated, based on claimant characteristics, a predicted probability of benefit exhaustion that was used to target the demonstration services.[3] The predicted probability of UI benefit

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[3]The predicted probability is based on a statistical model relating the probability of benefit exhaustion to claimant characteristics. See Appendix B for a detailed description of this model, which is used to target the demonstration services to claimants with the highest expected probabilities of exhaustion.

exhaustion was higher for D.C. claimants--71.1 percent compared with 51.3 percent for the Florida claimants.[4] This difference is a reflection of the traditionally high rate of benefit exhaustion in D.C. In 1990, for example, 53 percent of all D.C. claimants exhausted their benefits, compared with 42 percent of Florida claimants.

2. By Site in Florida

Within Florida, the demonstration eligible population varied greatly across the sites. One apparent difference was the age of the claimants. Claimants in Fort Lauderdale, Davie, Hialeah, Miami, and Clearwater tended to be older than the claimants in the other sites. The average age of claimants in these five sites ranged from 44 to 47 years, compared with 39 years in Pensacola and 41 to 42 years in St. Augustine, Orlando, Lakeland, and Fort Pierce.

The racial and ethnic background of claimants also varied widely. More than 90 percent of the claimants in Clearwater were white and non-Hispanic, compared with less than 20 percent of Miami claimants and less than 10 percent of Hialeah claimants. Sites with high proportions of African American claimants included Fort Pierce (25.7 percent), Miami (24.7 percent), Pensacola (24.1 percent), and Lakeland (19.2 percent). Sites with high proportions of Hispanic claimants included Hialeah (83.4 percent), Miami (58.1 percent), Davie (19.9 percent), and Orlando (18.7 percent).

Some of the Florida sites served claimant populations with limited educational backgrounds. In Hialeah and Miami, more than 40 percent of the claimants determined to be eligible for the demonstration had not completed high school. College completion was less than 5 percent among Hialeah and Fort Pierce claimants. Educational attainment was higher in other Florida sites. The high school dropout rate was lowest among claimants in Pensacola (14.2 percent) and Orlando (15.0 percent). College completion was more than 10 percent among claimants in Clearwater, Orlando, Fort Lauderdale, and Davie.

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[4]The predicted probabilities of exhaustion were calculated for each claimant as part of the demonstration eligibility determination.

Although the proportion of claimants from manufacturing in Florida was low overall, more than 20 percent of the claimants in Lakeland and Hialeah were from manufacturing. In contrast, less than 10 percent of Pensacola claimants were from manufacturing. Pensacola had a much higher proportion of claimants from public administration (15 percent) than other Florida sites (less than 5 percent). This difference is probably because of the presence of major military bases near Pensacola, whose employees would be classified as being in public administration. Since the demonstration was operating during a time of military downsizing, a substantial proportion of the Pensacola claimants were former employees at the military bases. Fort Pierce and Lakeland were noteworthy for having relatively high proportions of agricultural and mining workers--10 percent in Fort Pierce and 4 percent in Lakeland. No other site had more than 2 percent of claimants from agriculture and mining.

III. PARTICIPATION, TIMING OF SERVICES, AND CUSTOMER SATISFACTION

Understanding claimants' participation in services is important for two reasons. First, data on participation in demonstration services and the timing of services provides important information on the extent to which the demonstrations were administered as designed. Unusually low participation, participation in services out of sequence, or delays in participation would indicate that administration of the demonstrations was not entirely successful. More importantly, each of these potential problems would suggest that the implemented demonstrations may be testing a service package that differs from the one intended, and that estimated demonstration net impacts should be interpreted cautiously. Second, knowing how many claimants participated in specific services and the types of claimants that participated is also important for interpreting estimated net impacts. If more or fewer claimants had participated in services or if different types of claimants had participated, then the estimated net impacts might have been different. In addition, understanding what services claimants felt helped them the most is important for designing more efficient services for ongoing programs.

We find that the demonstration was generally implemented as designed. A substantial proportion of claimants assigned to the structured JSA (SJSA) treatment attended each of the service components, and the timing of each component was generally as expected. A substantial proportion of claimants in the individualized JSA treatments (IJSA and IJSA+) attended the orientation and assessment interview, and the assessment interview occurred shortly after the orientation, as designed. Relatively few claimants in the individualized programs attended the job search workshop or testing. While it is not surprising that participation rates in the individualized programs were lower than rates in the structured program, since the services were provided on an as-needed basis

in the individualized programs, they were lower than expected. It was anticipated that demonstration staff would be more aggressive in assigning claimants to services.

Relatively few claimants in the IJSA+ treatment were referred to or received training. Reluctance on the part of EDWAA staff to aggressively serve claimants in this treatment by providing greater access to limited training resources may explain this finding. The proportion of claimants who received training was nearly equal in all treatment groups, indicating that the EDWAA training component of the IJSA+ treatment was not fully implemented as designed. In both states, however, the proportion of claimants who received training was higher in the treatment groups than in the control group. Hence, the demonstration increased the frequency of EDWAA training receipt. Relatively few claimants in the demonstrations received services outside the demonstrations that were similar to those provided as part of the demonstrations.

Overall, participation in services was higher in D.C. than in Florida. This difference in participation rates arose primarily because claimants in Florida went back to work more rapidly than claimants in D.C. Claimants in Florida were also less likely to be assigned to specific services and were more likely to be excused if they were assigned to a service than were claimants in D.C. However, the primary reason claimants did not attend services was because they were working, and the primary reason claimants were excused was because they were job attached (either found a job or had been placed on recall). Overall, failure to participate without being excused was very low.

Demonstration participants were generally satisfied with the services they received. More than three-quarters of demonstration participants reported that they were very or somewhat satisfied with the JSA treatment they received. Participants in D.C. reported somewhat greater satisfaction than participants in Florida. There is no consistent difference in reported satisfaction by treatment group.

However, substantial proportions of claimants were not satisfied with the job search workshop for helping them find job openings.

Participation in the JSA treatments and in specific services varied consistently along several claimant characteristics. Claimants with more education, job tenure, and prior employment in manufacturing industries and in professional occupations were more likely than other claimants to participate in the demonstration services in both Florida and D.C.--that is, claimants from these groups made up a larger proportion of the claimants participating in services than of the claimants not participating. They were also more likely to participate in each of the specific demonstration services. In contrast, claimants with prior employment in construction industries and blue-collar occupations were less likely to participate.

We begin with a description of the services provided as part of the demonstrations and the process by which they were provided in each of the treatment groups. We then discuss assignment to and participation in services and the timing of services by treatment group for each state. Subsequent sections cover training services received outside of the demonstrations, claimants' satisfaction with the services received, and finally, reasons claimants gave for not participating in assigned services.

A. SERVICES PROVIDED IN JSA TREATMENTS

The types of services demonstration participants received and the process by which participants were assigned to specific services varied across treatment groups. Claimants assigned to the SJSA package were expected to complete a preset sequence of services. After receiving a notification letter asking them to report to their local office for JSA services, SJSA participants began their treatment by attending a group orientation session at about the seventh week after filing their initial claim. After the orientation, participants in SJSA attended aptitude and interest testing. Following

testing, claimants attended a job search workshop to help them formulate an adequate employment search plan. In an assessment interview following the workshop, a demonstration staff member reviewed the search plan and test results. Unless additional counseling was necessary, claimants in the structured treatment then proceeded with their self-guided job search. Periodic follow-up contacts with demonstration staff at four and eight weeks after orientation were also required. This sequence of activities was mandatory unless demonstration staff excused claimants from participation in the demonstration or from specific activities, or unless claimants returned to work.

The IJSA and IJSA+ treatments used the same basic services available for SJSA. However, the method of service delivery was different, in that the individualized treatments were intended to target specific services to individuals based on their needs. At the orientation sessions, ISJA and ISJA+ claimants received an overview of the available services and signed up for an individual assessment interview, at which time an individual service plan was developed. This plan was tailored to claimants and their specific situation. Demonstration services (such as testing, the job search workshop, or additional counseling and follow-up contacts) included in the individualized service plan were mandatory.

Service delivery for IJSA+ participants followed the same individualized approach as IJSA, with the additional option of a training component to upgrade the skills of claimants who needed this service. The objective of this treatment was to create a one-stop-shopping environment where claimants would receive information about EDWAA training opportunities at the same office that provided job search assistance. An EDWAA staff member described EDWAA training opportunities during the orientation session. A Job Service staff member reviewed the available types of training with each claimant again during the assessment interview. Claimants interested in EDWAA training

were to meet with EDWAA staff to discuss the training opportunities that interested them. Participation in training services was strictly voluntary.

B. ASSIGNMENT AND PARTICIPATION

In this section, we present information on assignment to specific JSA services, excusal from attending services, and participation in services by state. Participation in the JSA treatments and in specific services varied consistently along several claimant characteristics. Among eligible claimants, those with more education, job tenure, and prior employment in manufacturing industries and in professional occupations were more likely to participate in the demonstration in both Florida and D.C. They were also more likely to participate in each of the specific demonstration services. In contrast, claimants with prior employment in construction industries and blue-collar occupations were less likely to participate. Older claimants were more likely to participate in Florida and less likely to participate in D.C., and blacks were more likely to participate in D.C. and less likely to participate in Florida.

Because the process of assignment to services and the timing of services differ for the structured and individualized JSA groups, we discuss them separately. We begin with a presentation of results for the structured groups and then discuss results for the individualized groups. Data on participation in each service and the timing of services were collected on the PTS.

1. Structured JSA

a. Orientation

Most claimants assigned to the JSA demonstration attended the initial orientation, but participation was substantially higher in D.C. than in Florida. As shown in the top panel of Table IV.1, about 62 percent of the claimants assigned to the structured JSA program in Florida attended

TABLE III.1

SERVICE ASSIGNMENT AND ATTENDANCE, STRUCTURED JOB SEARCH ASSISTANCE GROUPS BY STATE

	JSA Demonstration		
	District of Columbia	Florida	New Jersey Demonstration
Orientation			
Number assigned	2,026	3,032	
Percent excused	4.8%	28.2%	
Percent did not attend*	16.4%	9.7%	
Percent attended	78.8%	62.0%	76.8%
Testing			
Number assigned	1,558	1,653	
As percent of orientation attendees	97.6%	87.9%	
Percent excused of those assigned	11.3%	31.6%	
Percent did not attend of those assigned	6.4%	3.3%	
Percent attended of those assigned	81.0%	79.1%	
As percent of orientation attendees	79.1%	69.5%	59.2%
As percent of all orientation assignees	62.3%	43.1%	45.5%
Workshop			
Number assigned	1,535	1,743	
As percent of orientation attendees	96.2%	92.7%	
Percent excused of those assigned	11.9%	27.8%	
Percent did not attend of those assigned	7.5%	4.2%	
Percent attended of those assigned	79.1%	76.5%	
As percent of orientation attendees	76.1%	70.9%	64.8%
As percent of all orientation assignees	59.9%	43.9%	49.8%
Assessment			
Number assigned	1,332	1,471	
As percent of orientation attendees	83.5%	78.2%	
Percent excused of those assigned	3.0%	4.1%	
Percent did not attend of those assigned	.8%	.3%	
Percent attended of those assigned	96.4%	93.7%	
As percent of orientation attendees	80.5%	73.3%	73.2%
As percent of all orientation assignees	63.4%	45.5%	56.2%

SOURCE: JSA demonstration participant tracking system; Corson, et al. (1989).

NOTE: Percentages for those assigned may not add up to 100% because some claimants who did not attend the orientation attended services, because many claimants coded as excused were not coded as assigned to services, and because some claimants assigned had missing codes for attendance.

*Includes claimants who did not attend or who were still coded as rescheduled at the end of the demonstration.

the orientation, while nearly 80 percent of claimants assigned in D.C. attended the orientation.

Attendance rates in Florida were lower than those observed for D.C. because claimants in Florida had shorter average spells of UI receipt than claimants in D.C. did, suggesting that a higher proportion of Florida claimants left UI prior to the orientation. Average duration on UI was about 16 weeks in Florida versus 20 weeks in D.C. Most of those leaving UI returned to work. About 76 percent of claimants in Florida who left UI prior to exhausting their benefits (excluding those excused from the demonstration) worked in the quarter following their exit. The corresponding figure for D.C. is about 60 percent. In addition, a larger proportion of the claimants in Florida was excused from the orientation than in D.C. More than 28 percent of the claimants assigned to the structured JSA program in Florida were excused, while less than 5 percent of those in D.C. were. The primary reason claimants were excused from the orientation session was because of re-employment or job recall.

Compliance with the directive to attend the orientation was reasonably high in both states. Less than 10 percent of claimants assigned to the structured JSA program in Florida did not attend the orientation, while about 16 percent did not attend in D.C., as shown in Table III.1. After excluding those who were excused from the orientation, attendance rates in the two states are more similar. In Florida, 86 percent of those not excused attended orientation, as compared with 83 percent in D.C.

b. Testing

As expected, assignment to testing was almost universal in the structured JSA programs. As shown in the second panel of Table III.1, almost 98 percent of those who attended the orientation in D.C. were assigned to testing, and about 88 percent of those who attended the orientation in Florida were assigned to testing. As was the case with orientation, a greater proportion of claimants in this treatment in Florida were excused from testing than in D.C. Almost one-third of the

claimants who were assigned to testing in Florida were excused, while only about 11 percent of those assigned to testing in D.C. were excused.

Attendance, as a percentage of those assigned to testing, was fairly high in both states. About 80 percent of those assigned to testing in both states attended. About 3 percent of those assigned to testing in Florida failed to attend, while about 6 percent in D.C. failed to attend. Because of a higher excusal rate, participation in testing among those who attended the orientation was lower in Florida than it was in D.C. About 70 percent of the claimants in Florida who attended the orientation received testing services. The comparable figure for D.C is 79 percent.

c. Job Search Workshop

Assignment to the job search workshop was nearly universal in the structured JSA program in both states. As shown in the third panel of Table III.1, among those who attended the orientation, 93 percent of claimants in Florida and 96 percent of claimants in D.C. were assigned to the job search workshop. As was the case with the orientation and testing, excusal from the workshop was more common in Florida than in D.C. More than one-quarter of claimants assigned to the workshop in Florida were excused from participating in the workshop, compared with about 12 percent in D.C. Very few claimants assigned to and not excused from the workshop failed to attend. In Florida, 4 percent of those assigned failed to attend; in D.C. about 8 percent failed to attend.

About three-quarters of the claimants in both states who were assigned to the workshop attended. Because of the higher excusal rate in Florida, the attendance rate at the workshop among those who attended the orientation was somewhat lower in Florida than in D.C. In Florida, 71 percent of the claimants who attended the orientation attended the job search workshop, while in D.C. 76 percent attended the workshop.

d. Assessment

Assignment to the assessment interview in the structured JSA program was somewhat lower than assignment to either testing or the job search workshop. As shown in the last panel of Table III.1, about 80 percent of claimants in both states who attended the orientation were assigned to an assessment interview, and about 95 percent of those assigned to assessment attended. Among those who attended the orientation, attendance at the assessment interview was somewhat lower in Florida than in D.C. Specifically, about 73 percent of the claimants who attended the orientation in Florida attended assessment, while over 80 percent attended assessment in D.C.

e. Comparison with the New Jersey Demonstration

The SJSA treatment was modeled after the New Jersey demonstration, which was conducted in 1986-1987. The SJSA treatment offered structured mandatory services similar to those offered in New Jersey. Given the similarities, we can make comparisons between the service participation rates in New Jersey, D.C., and Florida. This exercise can give us some idea of what variation might be expected among states in an ongoing program, as well as provide some context for interpreting any differences in net impacts between the demonstrations.

The orientation attendance rate in D.C. (79 percent) was similar to that in the New Jersey demonstration (77 percent), while the Florida rate was lower (62 percent), as shown in Table III.1. As discussed earlier, the lower attendance rate in Florida can probably be attributed to the higher orientation excusal rate, which was largely due to more rapid re-employment. Attendance rates for services beyond orientation, as a percentage of all claimants assigned to the treatment, tended to be higher in the D.C. and lower in Florida than in New Jersey. For example, about 60 percent of D.C.

claimants participated in the job search workshop, compared with 50 percent in New Jersey and 44 percent in Florida.

2. Individualized JSA

As discussed earlier, the ordering of services differed in the structured and individualized JSA treatments. In the individualized JSA treatments, the assessment interview occurred immediately after the orientation, before testing and the job search workshop, rather than after testing and the workshop, as was the case in the structured JSA treatments. Because of these differences, the order of presentation in this section differs from the section describing participation and timing of services in the structured JSA programs.

a. Orientation

Consistent with the findings for SJSA, attendance at the orientation in IJSA and IJSA+ was high for all groups and higher in D.C. than in Florida. As shown in the top panel of Table III.2, more than three-quarters of those assigned to the orientation in D.C. attended the orientation, compared with 66 percent in Florida. The comparable figures for the SJSA treatments were 79 percent for D.C. and 62 percent for Florida. Thus, participation in the orientation was similar for all treatments in D.C., but somewhat higher for IJSA and IJSA+ than for SJSA in Florida.

As was the case for SJSA, attendance in Florida was lower because claimants in Florida tended to have shorter UI spells, suggesting that a higher proportion of Florida claimants may have left UI before their scheduled orientation. Moreover, a follow-up survey indicates that about 90 percent of those in Florida who did not attend the orientation reported that they had gotten a job before their scheduled orientation; about 78 percent of those surveyed in D.C. gave this reason. In addition, a substantially larger proportion of claimants in the individualized JSA treatments were excused from

TABLE III.2

SERVICE ASSIGNMENT AND ATTENDANCE, INDIVIDUALIZED JOB SEARCH ASSISTANCE GROUPS, BY STATE

	District of Columbia		Florida	
	IJSA	IJSA+	IJSA	IJSA+
Orientation				
Number assigned	2,002	2,011	3,007	2,989
Percent excused	6.9%	5.3%	24.1%	24.8%
Percent did not attend*	15.9%	17.3%	9.1%	9.5%
Percent attended	77.2%	77.4%	66.7%	65.8%
Assessment				
Number assigned	1,662	1,587	1,954	1,913
As percent of orientation attendees	107.5%	102.0%	97.4%	97.3%
Percent excused	6.1%	3.8%	3.9%	3.6%
Percent did not attend*	.6%	.3%	.4%	.4%
Percent attended	91.3%	94.3%	95.4%	94.8%
As percent of orientation attendees	98.1%	96.2%	92.9%	92.3%
As percent of orientation assignees	75.8%	74.4%	62.0%	60.7%
Testing				
Number assigned	14	18	406	314
As percent of orientation attendees	0.9%	1.2%	20.2%	16.0%
Percent excused	0.0%	11.1%	13.8%	14.6%
Percent did not attend*	7.1%	0.0%	3.0%	4.5%
Percent attended	78.6%	72.2%	81.5%	80.9%
As percent of orientation attendees	0.7%	0.8%	16.5%	12.9%
As percent of orientation assignees	0.5%	0.6%	11.0%	8.5%
Workshop				
Number assigned	28	19	328	306
As percent of orientation attendees	1.8%	1.2%	16.3%	15.6%
Percent excused	3.6%	15.8%	22.9%	22.5%
Percent did not attend*	21.4%	15.8%	7.0%	4.6%
Percent attended	60.7%	42.1%	74.4%	79.4%
As percent of orientation attendees	1.1%	0.5%	12.2%	12.4%
As percent of orientation assignees	0.8%	0.4%	8.1%	8.1%
Counseling				
Number assigned	789	767	178	142
As percent of orientation attendees	51.0%	49.3%	8.9%	7.2%
Percent attended	95.6%	95.6%	3.8%	98.6%
As percent of orientation attendees	48.8%	47.2%	8.3%	7.1%
As percent of orientation assignees	37.7%	36.5%	5.6%	4.7%

SOURCE: JSA demonstration participant tracking system.

NOTE: Percentages for those assigned may not add up to 100% because some claimants who did not attend the orientation attended services, because many claimants coded as excused were not coded as assigned to services, and because some claimants assigned had missing codes for attendance.

*Includes claimants who did not attend or who were still coded as rescheduled at the end of the demonstration.

the orientation in Florida. Almost 25 percent of claimants in Florida were excused from the orientation, while only about 5 percent were excused in D.C. These figures are similar to those observed for claimants in the structured JSA programs. The primary reason claimants were excused was because they had obtained employment or been placed on job recall.

Compliance with the directive to attend the orientation was high in both states. Less than 10 percent of the claimants in the individualized JSA programs in Florida assigned to the orientation failed to attend, while about 15 percent failed to attend in D.C. After excluding claimants who were excused from attending the orientation, attendance rates in the two states are more similar. In Florida, almost 88 percent of those assigned to the orientation, who were not excused, attended. In D.C., about 82 percent of those who were not excused attended the orientation. These figures on compliance and attendance among those who were not excused are nearly identical to those observed for claimants in SJSA in each state.

b. Assessment

Assignment to the assessment interview was nearly universal in IJSA and IJSA+. As shown in the second panel of Table III.2, in Florida, 97 percent of claimants who attended the orientation were assigned to assessment. In D.C., more claimants were assigned to assessment than attended the orientation, indicating that some claimants may have been assigned to an assessment interview without attending the orientation. These figures are higher than those observed for claimants in SJSA, as was expected given that the assessment interview occurred much earlier in IJSA and IJSA+ than it did in SJSA.

Consistent with the findings for SJSA, very few claimants assigned to assessment in IJSA or IJSA+ were excused or failed to attend the assessment interview. In both D.C. and Florida, the attendance rate exceeded 90 percent. Attendance among those not excused exceeded 95 percent.

These figures are nearly identical to those observed for SJSA and show that there was nearly universal compliance with the directive to attend the assigned assessment interview. One reason attendance at assessment was high among claimants who attended orientation was that assessment sometimes happened on the same day as orientation in the IJSA and IJSA+ treatments.

Attendance, as a percentage of those attending the orientation, is somewhat higher in D.C. than Florida. About 97 percent of claimants who attended the orientation attended assessment, while the comparable figure for Florida is about 92 percent. Attendance rates at the assessment interview are lower in SJSA (73 percent for Florida and 81 percent for D.C.) than in IJSA and IJSA+ in both D.C. and Florida. This finding is not surprising. As noted at the beginning of the chapter, assignment to the assessment interview occurred later in the structured JSA treatment than it did in the individualized JSA treatments. For this reason, more claimants in SJSA were expected to exit UI before the assessment interview.

c. Testing

Assignment to testing was very low in the individualized JSA programs. As shown in the third panel of Table III.2, about 20 percent of claimants in IJSA who attended the orientation were assigned to testing in Florida, and about 16 percent in IJSA+ were assigned to testing. About 1 percent of claimants in the individualized JSA programs in D.C. who attended the orientation were assigned to testing. These figures for testing assignment are much lower than those for SJSA.

It is not surprising that assignment to testing is much lower in the individualized treatments. Testing was part of the package of services provided to all claimants in SJSA, while in the individualized treatments, only claimants who were determined to need testing were assigned to this service. Assignment to testing in the individualized treatments was lower in D.C. than in Florida. Staff in D.C. did, however, provide one-on-one counseling to many claimants (see the bottom panel

of Table III.2). D.C. staff may not have aggressively assigned claimants to group services because they felt that one-on-one counseling was more effective or more acceptable to demonstration participants and because of a shortage of resources for providing group services. The D.C. office had difficulty maintaining sufficient staff who were trained to conduct group services and had a shortage of space for providing group services. In contrast, the D.C. office had ample staff for one-on-one counseling and adequate office space for conducting one-on-one services.

As was the case with assessment, very few claimants assigned to testing were excused or failed to attend. Consequently, attendance among those assigned to testing was high. About 80 percent of those assigned to testing in the individualized treatments attended. The only exception is IJSA+ in D.C., where 72 percent of those assigned to testing attended. However, figures for the D.C. programs are based on small numbers and should be interpreted with caution. These figures show that there was high compliance with the directive to attend testing. Among those not excused, attendance is very high. Almost 95 percent of those not excused in Florida attended testing, and about 80 percent of those not excused in D.C. attended testing. Attendance rates, as a percentage of claimants attending the orientation, indicate that testing was rare in the individualized JSA programs. Among those attending the orientation, about 15 percent of claimants in Florida received testing services, and less than 1 percent were tested in D.C.

d. Job Search Workshop

As was the case with testing, assignment to the job search workshop was relatively rare in the individualized JSA treatments. As shown in the fourth panel of Table III.2, about 16 percent of claimants in the individualized treatments in Florida and less than 2 percent of claimants in D.C. were assigned to the workshop. Considerably fewer claimants were assigned to the workshop in IJSA and IJSA+ than were assigned in SJSA, because only claimants deemed in need of the job

search workshop were assigned in the individualized programs. Again, assignment to the workshop was lower in the individualized treatments in D.C than in Florida. However, D.C. conducted one-on-one counseling for many claimants in the individualized treatments and may have provided claimants with information similar to that provided in the workshop.

Almost a quarter of those assigned to the job search workshop in the Florida individualized treatments were excused from attending. About three-quarters of those assigned to the workshop in Florida attended, and almost 100 percent of those not excused attended the workshop in Florida. These figures are similar to those observed for claimants in SJSA in Florida.

Attendance rates, as a percentage of those attending the orientation, are low for the individualized JSA treatments. About 12 percent of those who attended the orientation attended a workshop in Florida, and about 1 percent attended in D.C. These results are similar to those reported above for testing in IJSA and IJSA+ and are much lower than those found for SJSA.

e. Counseling

In addition to the services discussed above, claimants in the individualized JSA programs, who were deemed to be in need, could be assigned to additional one-on-one counseling. Assignment to counseling was much more common in D.C. than in Florida. As shown in the last panel in Table III.2, less than 10 percent of claimants who attended the orientation in Florida were assigned to additional counseling, compared to about 50 percent of those in D.C. As noted earlier, in D.C. claimants were more likely to receive one-on-one services instead of group services like testing and the job search workshop. This difference in how claimants were assigned to services is important for interpreting state differences in estimated net impacts for the individualized JSA programs.

As was the case with other services, attendance among those assigned to counseling was very high, 94 percent or higher. Attendance rates, as a percentage of claimants attending the orientation,

were much higher in D.C. Almost one-half of the these claimants in the individualized programs in D.C attended additional counseling, compared with less than 10 percent in Florida. So D.C. appears to have compensated for the lack of group services among the IJSA and IJSA+ claimants by providing a large volume of one-on-one counseling.

C. TIMING OF SERVICES

The demonstrations were successful in achieving early intervention. The timing of initial services in the JSA treatments was generally consistent in both states with the original demonstration design. This was true for both the structured and individualized treatments.

1. Structured JSA

As shown in Table III.3, the mean time between the beginning of the benefit year and the orientation was 49 days (7 weeks) in Florida and 46 days (6.6 weeks) in D.C., both very close to the expected time of seven weeks. About 80 percent of claimants in each state attended the orientation within 8 weeks or less, and very few claimants had lengthy delays before the start of participation. Only 6 percent in Florida and 3 percent in D.C. attended the orientation more than 10 weeks after the beginning of their benefit year.

The longer time to services in Florida arose partly because of design differences between the two demonstrations. Not all Florida demonstration offices had enough participants to offer initial demonstration services on a weekly basis. For this reason, services were delivered on a bi-weekly schedule in Florida. A bi-weekly schedule implies that half of claimants in Florida waited an extra week before participating in the orientation. In D.C., demonstration services were provided on a weekly basis. This difference in the scheduling of services accounts for the three-day difference in the timing of the orientation.

TABLE III.3

TIMING OF SERVICES, STRUCTURED JOB SEARCH ASSISTANCE GROUPS, BY STATE

Time Between Services	JSA Demonstration		New Jersey Demonstration
	District of Columbia	Florida	
Beginning of Benefit Year to Orientation			
Less than 6 weeks	42.9%	22.1%	
6 to 8 weeks	42.1%	55.9%	
8 to 10 weeks	12.2%	16.5%	
More than 10 weeks	2.8%	5.5%	
Mean in days	46	49	35
Orientation to Testing			
Less than 1 week	15.8%	87.2%	
1 to 2 weeks	75.1%	4.2%	
More than 2 weeks	9.1%	8.6%	
Mean in days	9	3	3
Testing to Workshop			
Less than 1 week	92.9%	89.2%	
1 to 2 weeks	4.4%	1.2%	
More than 2 weeks	2.7%	9.6%	
Mean in days	2	5	8
Workshop to Assessment			
Less than 1 week	76.9%	33.1%	
1 to 2 weeks	16.5%	57.2%	
More than 2 weeks	6.6%	9.7%	
Mean in days	5	8	4

SOURCE: JSA demonstration participant tracking system; Corson et al. (1989).

Following orientation, the remaining services occurred in quick succession. More than 90 percent of claimants attended testing within two weeks of their orientation. After testing, about 90 percent attended the job search workshop within less than one week. Assessment then occurred within about a week after the workshop.

Services were generally initiated earlier in a claimant's UI spell in the New Jersey demonstration than in the JSA demonstration. In the New Jersey demonstration, the orientation was targeted for the fifth week of a claimant's UI spell, while in the JSA demonstration the orientation was designed to occur in about the seventh week. Average time from the beginning of the benefit year to the orientation was only 35 days in the New Jersey demonstration, which is more than one week shorter than the corresponding period in D.C. or Florida. There are two reasons why services began earlier in New Jersey. First, UI staff in Florida and D.C. felt they needed two weeks to mail the letter telling claimants to attend the orientation, while New Jersey did it in one week. Second, a bi-weekly schedule for providing services was used in Florida, while New Jersey and D.C. used a weekly schedule. The remaining mandatory services were delivered shortly after orientation in all three states.

2. Individualized JSA

The timing of initial services in the individualized JSA treatments was consistent with the original demonstration design. As shown in Table III.4, the mean time from the beginning of the benefit year to the IJSA orientations was identical to that for SJSA--49 days in Florida and 46 days in D.C. As was the case in the structured programs, about 80 percent attended the orientation within 8 weeks of the beginning of the benefit year and very few had lengthy delays: only about 5 percent

TABLE III.4

TIMING OF SERVICES, INDIVIDUALIZED JOB SEARCH ASSISTANCE GROUPS, BY STATE

	District of Columbia		Florida	
Time Between Services	IJSA	IJSA+	IJSA	IJSA+
Beginning of Benefit Year to Orientation				
Less than 6 weeks	36.0%	41.4%	21.7%	22.5%
6 to 8 weeks	50.1%	42.4%	56.7%	56.3%
8 to 10 weeks	12.2%	11.8%	16.4%	16.5%
More than 10 weeks	1.7%	4.4%	5.2%	4.7%
Mean in days	46	46	49	49
Orientation to Assessment				
Less than 1 week	90.1%	85.4%	84.7%	88.9%
1 to 2 weeks	7.6%	8.5%	11.7%	8.7%
More than 2 weeks	2.3%	6.1%	3.6%	2.4%
Mean in days	2	4	3	3

SOURCE: JSA demonstration participant tracking system.

in Florida and about 3 percent in D.C. had delays of more than 10 weeks. The longer delay found for Florida is partly due to the bi-weekly service delivery used in that state. The time between the orientation and assessment was short -- an average of three days in both Florida and D.C. (two days for Treatment 2 and four days for Treatment 3). Very few claimants waited more than two weeks.

D. TRAINING

In addition to the other services, claimants could also receive on-the-job and classroom training. Participation in training services was strictly voluntary. In contrast to other groups, claimants assigned to the UISA+ group received more information about training programs, received their information directly from EDWAA staff, and were intended to have greater access to training service resources. However, as shown in Table III.5, relatively few claimants in the individualized program with training were referred to training. In Florida, about 8 percent of those who attended the orientation were referred to training. About 14 percent were referred to training in D.C.

Attendance among those referred to training is relatively low compared to participation in other services, especially for D.C. About 64 percent of those referred to EDWAA in Florida received training, while only about 12 percent of those referred in D.C. received training. The low rates are not too surprising because there are many reasons that claimants would have dropped out between referral and potential participation in EDWAA. For example, referred claimants might have been found ineligible for EDWAA training, failed to find an available training slot or a training opportunity that was convenient for them, or decided that training was not appropriate for them. The especially low rate in D.C. may indicate that there were considerable obstacles to EDWAA training in that state.

Overall, although EDWAA training participation among the demonstration participants was low, the demonstration increased the training rates. In Florida, 2.8 percent of the control group

TABLE III.5

TRAINING ASSIGNMENT AND ATTENDANCE BY JOB SEARCH ASSISTANCE GROUP AND STATE

	District of Columbia				Florida			
	SJSA	IJSA	IJSA+	Control	SJSA	IJSA	IJSA+	Control
Number Referred	0	0	218	0	0	0	156	0
As a percentage of those attending orientation	0.0%	0.0%	14.0%	0.0%	0.0%	0.0%	7.9%	0.0%
Number Attended	28	24	25	16	107	106	99	85
As a percentage of those assigned training	NA	NA	11.5%	NA	NA	NA	63.5%	NA
As a percentage of those assigned orientation	1.4%	1.2%	1.2%	0.8%	3.5%	3.5%	3.3%	2.8%

SOURCE: JSA demonstration participant tracking system.

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participated in training compared with 3.3 to 3.5 percent of the treatment groups. The difference in the training rate for the control group and the combined treatment groups is statistically significant at the 90 percent confidence level. The training rates in D.C. are lower than in Florida, but again the rates are higher for the treatment groups than for the control group. Only 0.8 percent of the control group in D.C. participated in training, compared with 1.2 to 1.4 percent for the treatment groups. Again, these differences are statistically significant at the 90 percent level.

Although the treatments had a positive effect on training participation, the training rate was not higher in the IJSA+ group than in the other treatment groups, despite the IJSA+ design that emphasized access to EDWAA training. This finding confirms the perceptions from the site visits that EDWAA staff were not providing claimants in the IJSA+ greater access to training resources. In effect, the IJSA+ treatment was not substantially different than the IJSA treatment in the field.

Results from the follow-up survey of claimants in all three treatments groups and in the control groups underscore the demand for more training.[1] About 60 percent of the claimants surveyed in Florida and about 70 percent of those surveyed in D.C. indicated they were interested in on-the-job training. Only 9 percent of those in Florida and 15 percent of those in D.C. interested in on-the-job training reported they had been offered training. About 50 percent of those in Florida offered on-the-job training attended training, while about 30 percent of those offered in D.C. attended. Overall, only 5 percent of those in Florida and D.C. who said they were interested in on-the-job training actually received such training.

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[1]The follow-up survey was conducted by phone approximately one year after a claimant's initial UI claim. It included questions about service participation, job search activities, pre-UI employment and earnings, post-UI employment and earnings, and personal characteristics. Appendix A discusses the results of the survey.

Nearly identical figures are reported for classroom training. About 60 percent in Florida and about 70 percent in D.C. said they were interested in classroom training. About 20 percent of those interested in Florida and about 33 percent of those interested in D.C. said they were offered classroom training. About 50 percent of those offered classroom training in Florida attended. The comparable figure for D.C. is 44 percent. Overall, about 10 percent of those interested in classroom training in Florida and about 15 percent of those interested in D.C. actually received classroom training.

E. SERVICES RECEIVED OUTSIDE THE DEMONSTRATION

Data on re-employment services received outside the demonstration were collected as part of the follow-up survey, which is described in Appendix A. These data are used to determine whether claimants in the control groups received more outside services than claimants in the treatment groups. The level of services each group received is important for interpreting the estimated net impact. If the control group received greater outside services there could be smaller estimated net impacts.

Information on the receipt of services outside the demonstration is displayed in Table III.6. Except for training and education, the results in Table III.6 show that claimants in the control group received more outside services than claimants in the treatment groups. For instance, about 13 percent of claimants in the control group in Florida report receiving outside services, while 3 to 5 percent of claimants in the treatment groups report outside services. In D.C., 17 percent of claimants in the control group and 4 to 6 percent of claimants in the treatment groups report receiving outside services. Similarly, 6 to 7 percent of claimants in the control group in both states report receiving JTPA services, while 1 to 2 percent of the claimants in the treatment groups in both states report receiving JTPA services. This same pattern is found in reports of receipt of specific services.

As is clear from these statistics, the control group reported receiving substantial services outside the demonstration. The comparison of outcomes for the treatment and control groups, therefore,

TABLE III.6

OUTSIDE SERVICES BY JOB SEARCH ASSISTANCE GROUP AND STATE

	District of Columbia				Florida			
	SJSA	IJSA	IJSA+	Control	SJSA	IJSA	IJSA+	Control
Percent Received Any Outside Services	5.7%	4.4%	4.1%	17.2%	5.0%	3.1%	5.0%	13.4%
Percent Participated in JTPA	1.4%	.9%	1.0%	6.7%	1.8%	0.6%	1.6%	5.9%
Percent Specific Services								
How to apply for jobs	3.1%	2.2%	2.6%	9.3%	2.3%	1.1%	3.1%	6.3%
Fill out applications	3.3%	1.8%	0.5%	6.2%	1.7%	0.3%	1.9%	5.5%
Career counseling	3.5%	3.1%	3.6%	10.5%	2.6%	0.6%	1.6%	7.5%
Testing	2.4%	1.8%	1.0%	7.2%	2.0%	0.3%	1.2%	6.7%
Information on training	2.4%	1.8%	2.1%	11.5%	2.8%	0.8%	3.1%	6.3%
Information on education	2.8%	2.2%	2.6%	8.4%	2.8%	0.6%	3.4%	7.3%
Percent Received Any Outside Training or Education	16.0%	18.0%	20.1%	17.0%	17.9%	18.6%	16.5%	15.6%
Mean Number of Training or Education Programs	1.37	1.44	1.23	1.55	1.25	1.18	1.30	1.48

SOURCE: JSA demonstration follow-up survey.

provides an estimate of the impacts of the three JSA service packages compared with outside services rather than compared with no services. If all services tended to reduce UI spells, greater receipt of outside services by control group members suggests that the estimated net impacts of the demonstration were smaller than they would have been had claimants in the control groups not received greater outside services. However, the proportion of control group members receiving outside services is still low relative to the proportion of treatment group members who received services either inside or outside the demonstration. Consequently, the effect of the availability of outside services on the estimated net impacts of the demonstration should have been small.

The second to last row of Table III.6 shows that claimants in the control groups were no more likely to receive training services outside the demonstration than were claimants in the treatment groups. The last row shows that controls did not participate in appreciably more training programs outside of the demonstration than did treatments. These findings suggest that receipt of outside training will have little effect on the estimated net impacts.

F. CUSTOMER SATISFACTION

The follow-up survey contained several questions designed to assess participants' satisfaction with the services they received as part of the JSA demonstration.[2] As shown in Table III.7, about one-quarter of those in Florida and about a third of those in D.C. who attended an assessment interview thought that it was very useful. About 40 percent in Florida, and about one-third in D.C., said the assessment interview was somewhat useful. About a third in each state said that the interview was not useful. Although the assessment interview was somewhat different in the structured and individualized JSA treatments and more claimants in the individualized treatments

[2]The follow-up survey is described in Appendix A.

TABLE III.7

CUSTOMER SATISFACTION BY JOB SEARCH ASSISTANCE GROUP AND STATE

	District of Columbia			Florida		
	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
Usefulness of assessment for planning work future						
Percent very	34.1	27.3	32.3	25.9	25.2	28.6
Percent somewhat	32.2	34.1	35.3	42.6	35.2	37.8
Percent not at all	33.6	38.6	32.3	31.6	39.6	33.6
Usefulness of workshop for finding job opening						
Percent very	26.7	23.6	15.3	22.2	17.3	21.8
Percent somewhat	33.1	34.7	35.6	34.7	39.1	26.8
Percent not at all	40.3	41.6	49.2	43.1	43.7	51.5
Usefulness of workshop for résumé						
Percent very	46.4	24.6	17.3	37.6	29.4	31.4
Percent somewhat	27.9	26.1	24.1	30.1	24.8	29.4
Percent not at all	25.7	49.4	58.6	32.3	45.8	39.2
Usefulness of workshop in helping define job skills and goals						
Percent very	38.3	18.9	25.4	34.6	29.1	27.5
Percent somewhat	32.0	43.2	25.4	32.2	29.1	36.3
Percent not at all	29.7	37.8	49.2	33.2	41.8	36.3
Usefulness of Job Search Assistance for finding a job						
Percent very	32.2	31.2	33.9	30.4	25.7	29.7
Percent somewhat	50.1	47.6	45.3	43.5	52.6	47.5
Percent not very	11.1	18.0	9.5	19.2	12.9	16.8
Percent not at all	6.6	3.2	11.3	7.0	8.9	5.9

SOURCE: JSA demonstration follow-up survey.

attended assessment than in the structured programs, there are no substantial treatment group differences in satisfaction with the assessment interview.

This same general pattern is observed for satisfaction with the job search workshop in helping participants find jobs. Although respondents report finding the workshop somewhat less helpful for finding a job than the assessment interview, there are no substantial differences in reports of the usefulness of the workshop for helping claimants find a job among treatment groups. In contrast, claimants in the structured JSA programs report greater satisfaction with the workshop with respect to helping them write a resume and helping them define their job skills and goals. The treatment group differences are somewhat larger in D.C. than in Florida. For instance, more than 45 percent of respondents in the structured program in D.C. report the workshop was very useful for help with resume writing, while about 20 percent of respondents in the individualized programs reported that the workshop was very useful for helping them with resume writing. This finding is somewhat surprising because many more claimants in the structured programs were required to attend the job search workshop.

As shown at the bottom of Table III.7, about 30 percent of claimants in both Florida and D.C. thought that JSA was very useful in helping them find a job. Almost 50 percent in both Florida and D.C. said that JSA was somewhat useful in helping them find a job. About 20 percent said that JSA was not very useful or not at all useful in helping them find a job. There are no substantial differences in reported satisfaction by treatment group. This finding is somewhat surprising because claimants in the structured programs were assigned to and received more services than claimants in the individualized programs.

G. REASONS FOR NOT ATTENDING

The follow-up survey also contained questions designed to examine reasons for service non-attendance. As shown in Table III.8, the primary reason that claimants gave for not attending the orientation was they had found a job before orientation. In Florida, 80 to 90 percent of nonattendees reported they found a job before their scheduled orientation, while in D.C. about 75 percent gave this response. The figures are somewhat lower for the structured program in Florida, and for the individualized program with training in D.C. About 5 percent in each state reported that they did not attend the orientation because they didn't think JSA would help them get a job.

Employment was also the most common reason claimants gave for not attending their scheduled job search workshop. This reason for not attending the workshop was given most often by claimants in the structured programs and given least often by claimants in the individualized program that included training. Because attendance rates for the workshop were high, relatively few respondents answered this question. Consequently, any treatment group differences must be interpreted with caution. About 20 percent in both Florida and D.C. report that they didn't attend the workshop because they didn't think it would help them find a job.

Employment was also the most common reason given for not attending the assessment interview. Almost 55 percent in Florida and 45 percent in D.C. report that they didn't attend assessment because they had gotten a job before the date of their assigned assessment interview. About a third of the claimants in Florida and D.C. report that the reason they did not attend was that they thought that the assessment interview wouldn't help them get a job. Because attendance rates for assessment were high, relatively few respondents were asked these questions, and for this reason, any treatment group differences should be interpreted with caution.

TABLE III.8

REASONS FOR NOT ATTENDING SERVICE BY JOB SEARCH ASSISTANCE GROUP AND STATE

	District of Columbia			Florida		
	SJSA	IJSA	IJSA+	SJSA	IJSA	IJSA+
Orientation						
Percent had job	79.6	78.3	71.4	81.5	88.7	90.8
Percent on recall	16.3	8.7	21.4	4.2	1.9	4.6
Percent in other program	2.0	4.4	0.0	7.6	1.9	1.5
Percent said service wouldn't help	2.0	8.7	7.1	6.7	7.6	3.1
Workshop						
Percent had job	46.2	25.9	12.3	34.9	23.4	18.4
Percent on recall	2.5	3.7	14.0	4.8	3.1	3.3
Percent in other program	2.5	14.8	5.2	19.1	11.1	6.7
Percent other excused	18.0	7.5	5.2	15.9	1.6	8.4
Percent said service wouldn't help	15.4	24.1	24.6	7.9	31.3	26.7
Percent other	15.4	24.1	38.6	17.5	29.3	36.7
Assessment						
Percent had job	51.9	30.0	28.6	65.0	39.1	50.0
Percent on recall	11.1	20.0	28.6	5.0	8.7	0.0
Percent in other program	7.4	10.0	14.3	5.0	8.7	7.1
Percent said service wouldn't help	25.9	40.0	28.6	25.0	43.5	42.9

SOURCE: JSA demonstration follow-up survey.

IV. COSTS OF THE INTERVENTION

An important aspect of the demonstration evaluation is to document and assess the costs of the three treatments. This information is essential for determining the degree to which each of the treatments would be cost-effective on an ongoing basis. Moreover, cost information is essential for planning and budgeting future programs.

Our primary objective in this chapter is to measure the costs of each treatment and to compare costs across the treatments and states. We attempt to explain costs differences between the states by linking these cost differences to differences in services in the two states. Our comparisons will also reveal whether, as expected, the individualized JSA treatments (IJSA and IJSA+) were less costly than the structured JSA treatment (SJSA) and the magnitude of the cost difference. The findings on costs presented in this chapter are also used as a basis for our evaluation of the cost-effectiveness of the JSA treatments, which is presented in Chapter X.

A. ESTIMATING PROGRAM COSTS

The cost of operating the demonstration included the local office cost of providing the initial services via the Job Service and the cost to EDWAA of having an EDWAA staff member involved in the demonstration presentations and assessments. There are also costs from having UI staff monitor compliance of claimants with the reporting requirements. Costs were also incurred in the central offices of D.C. and Florida to manage the demonstration, monitor the sites, and provide training to staff.

For both states, we have cost data that are based on staff time and other expenses that were charged to a demonstration account. These charges cover staff training, the pilot phase of the demonstration, and the full operation of the demonstration. In Florida both central office costs and local office costs were charged to the demonstration, so the total charges represent the full costs of the demonstration. In contrast,

D.C. did not charge central office costs directly to the demonstration--only demonstration office staff charged to the demonstration. In the remainder of our cost analysis, we assume that D.C. central office costs are equal to 18 percent of local office costs, which is the same assumption D.C. used in its accounting of program costs. EDWAA staff in either site did not charge their time to the demonstration, so we generate separate estimates of EDWAA costs.

Total demonstration charges in the two states were equal to $1,301,267 in D.C. and $1,356,659 in Florida. Our objective in this chapter is to transform these charges into measures of demonstration costs per claimant for each of the treatments in the two states. Ideally, we would like to generate estimates of costs per claimant of providing similar services to the treatments in an ongoing program. However, we have limited information for using our cost data to generate accurate estimates of the costs of an ongoing program. We have chosen, therefore, to calculate the costs of the intervention as it occurred in the demonstration, and we treat these as our baseline estimates of providing the service packages in an ongoing program. In our sensitivity analysis, we estimate the effect of various changes in our assumptions on the baseline estimates. These changes in assumptions are designed to account for ways in which costs of an ongoing program might differ from costs of the demonstration.

The first step in transforming the demonstration charges into per-claimant cost figures is to determine how the pilot phase should be treated in the transformation. In Florida, the pilot phase operated in a single office (Orlando). Accounting for the costs of the pilot phase in Florida is straightforward because the pilot phase operated in the same manner and with the same intensity as the full demonstration operations throughout the state. But accounting for the costs of the pilot phase in D.C. is less straightforward. In D.C., the pilot phase used the same office and staff as the full demonstration operations, but the pilot phase served only about a quarter of the claimants per week that were served during the full demonstration operations. Given this difference, the costs per claimant served during the pilot phase may have been different than the costs per claimant during the full demonstration operations.

Because of the different intensity of the pilot phase in D.C., we estimate costs for D.C. using two different approaches. Our primary estimates assume that the per-claimant costs were the same for the pilot phase as for the full demonstration. We also present alternative estimates that are based on the assumption that total costs per week of serving the pilot claimants were the same as the total costs per week of serving the full demonstration claimants, regardless of how many claimants were served in either case. In this case, the per-claimant costs would be higher during the pilot phase than during full demonstration operations because during the pilot phase fewer claimants were served for the same weekly costs. We will present the estimated costs per claimant for the full demonstration operations as our alternative estimates.

The second step in calculating per-claimant costs is to allocate the costs to the different treatments. Although each state charged costs directly to the demonstration, costs were not charged to separate treatments. Allocation of costs to different treatments is a key issue, however, because the individualized treatments (IJSA and IJSA+) were expected to be less expensive than the structured JSA treatment (SJSA). If some claimants did not need all of the services offered in SJSA, then an individualized approach, which tailors the services to claimants' needs, should be a cheaper method of providing services. To examine the evidence on this point, we would like to be able to estimate costs for the separate treatments to document the cost advantage of the individualized treatments. Since our data provide only cost figures for the demonstration as a whole, we need to develop a method for allocating the costs to treatments. Our chosen method is to estimate the staff time that was spent serving the average claimant in each of the treatment groups. Then based on the relative time spent serving claimants in the different treatment groups, we allocate the reported costs to the different treatments.

The estimates and assumptions used to allocate the demonstration costs to treatments were adapted to reflect the different approaches used to operate the demonstration in the two states. These figures, which are presented in Table IV.1, are the basis for our calculations of the staff time per claimant required to provide each service within each treatment. For example, Table IV.1 shows that the SJSA orientation in

D.C. was conducted by two JSA staff members, and we estimate that the average orientation lasted 0.75 hours. We assume that staff spent time on paperwork outside orientation that was approximately equal to time spent inside orientation. Performing the orientation therefore required 1.5 hours of two staff members' time, which yields total staff time needed to conduct the orientation of 3 person-hours. Since average attendance at a D.C. SJSA orientation was about 30 claimants, the orientation costs per attendant were 0.1 hours (3 person-hours divided by 30 claimants). For claimants who failed to attend the orientation, we assume time spent following up on them was equal to 0.25 hours per claimant. Given that the orientation attendance rate was 76 percent, the average claimant had a .76 chance of attending and having costs equal to 0.1 hours. Alternatively, the average claimant had a .24 chance of not attending and having costs equal to 0.25 hours. The orientation cost for the average claimant was therefore 0.136 person-hours [(.76 * 0.1)+(.24 * 0.25)].

TABLE IV.1

ESTIMATES AND ASSUMPTIONS USED TO ALLOCATE DEMONSTRATION COSTS TO TREATMENTS

Service Component	District of Columbia			Florida		
	SJSA	IJSA	IJSA +	SJSA	IJSA	IJSA +
Orientation						
Duration (hours)	0.75	0.75	1.25	0.50	0.50	1.00
Staff participating	2	2	2	1	1	1
Claimants attending	30	30	30	7	7	7
Claimant attendance rate (percent)	76	73	75	62	67	66
Paperwork multiplier[a]	2	2	2	2	2	2
Time spent per no-show (hours)	0.25	0.25	0.25	0.25	0.25	0.25
Testing						
Duration (hours)	4	4	4	4	4	4
Staff participating	2	2	2	2	2	2
Claimants attending	10	10	10	5.5	5.5	5.5
Claimant attendance rate (percent)	62	1	1	44	11	9
Time spent on paperwork (hours)	0.5	0.5	0.5	0.5	0.5	0.5

TABLE VI.1 (*continued*)
Page 2

Service Component	District of Columbia			Florida		
	SJSA	IJSA	IJSA +	SJSA	IJSA	IJSA +
Job Search Workshop						
Duration (hours)	15	15	15	17	17	17
Staff participating	2	2	2	2	2	2
Claimants attending	25	25	25	5.5	5.5	5.5
Claimant attendance rate (percent)	59	1	0	45	8	8
Time spent on paperwork (hours)	2	2	2	2	2	2
Assessment						
Duration (hours)	0.75	1.25	1.25	0.50	1.25	1.25
Claimant attendance rate (percent)	63	74	76	46	63	61
Follow-Up Sessions						
Duration (hours)	0.5	0.5	0.5	0.167	0.0	0.0
Claimant attendance rate (percent)	63	74	76	45	0	0

[a]To account for paperwork associated with orientations, we double all the staff time estimates associated with conducting orientations — that is, we assume a one hour orientation requires one hour of paperwork.

We made similar calculations for all of the services for each treatment. The estimates for each service were summed to yield an estimated total staff time spent per claimant for each treatment. These treatment totals were then used to allocate the actual charges across the treatments.

Some of the differences between D.C. and Florida that are shown in Table IV.1 are the result of different operations in the two states. For example, in Florida, claimants were offered services in their local offices, so the number of claimants participating in group services was relatively small in the offices with small caseloads. In D.C., all claimants were offered services in a centralized demonstration office, and a large number of claimants were served each week. Hence, the number of participants in the typical group service, such as testing or the job search workshop, tended to be higher in D.C. than in Florida. For example, the estimated average attendance at testing was 10 claimants in D.C. compared with 5.5 claimants in Florida. Similarly, the estimated average attendance at the job search workshops was 25 claimants in D.C. compared with 5.5 claimants in Florida.

The difference in attendance rates between the states is especially important in estimating the staff time required for group services. Given this difference, the costs of the group services in SJSA were spread across a larger number of claimants in D.C. than in Florida, and this has some important implications for differences in the estimated costs per claimant in D.C. and Florida. First, the staff time per claimant to provide group services tended to be lower in D.C. than in Florida. Hence, the costs of SJSA, which was composed largely of group services, tended to be lower relative to IJSA and IJSA+ in D.C. than in Florida.

B. COST ESTIMATES

Using the estimates and assumptions presented in Table IV.1, we estimated staff time per claimant for each of the services in each treatment. By combining these estimates of the different services, we derived an estimate of staff time used per claimant for each treatment group. The total demonstration costs

were then allocated to the treatments in proportion to the relative estimates of staff time needed for each treatment. These per-claimant cost estimates are presented in Table IV.2.

As expected, the costs per claimant of SJSA exceeded the costs of IJSA and IJSA+ in both states. In D.C., the SJSA costs were $286 per claimant, compared with $199 and $216 for IJSA and IJSA+. In Florida, the SJSA costs were $241 per claimant, compared with $97 and $103 for IJSA and IJSA+.

These estimates also imply that the D.C. demonstration was more costly than the Florida demonstration. All of the D.C. treatments were more expensive than the Florida counterparts. The primary reason for the difference probably is that staff are more highly paid in D.C. than in Florida, which at least partly reflects the higher cost of living in D.C. The D.C. treatments also tended to have higher participation rates (see Chapter III), so more services were provided in D.C. Furthermore, D.C. reported providing substantial one-on-one counseling, which may have further added to the resources used in the demonstration. This high level of services translated into large amounts of staff time charged to the demonstration. D.C. also spent significant staff time following up on claimants assigned to IJSA or IJSA+ after they had completed their scheduled services. In contrast, Florida conducted no follow-up with IJSA or IJSA+ claimants.[1]

The differences in costs between SJSA and the two IJSA treatments were larger in Florida than D.C. In dollar terms, SJSA costs were about $140 higher than the average of the IJSA and IJSA+ costs in Florida, while the comparable difference in D.C. was roughly $80. In percentage terms, SJSA costs were about 140 percent higher than the IJSA and IJSA+ costs in Florida, compared with

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[1] Florida chose not to follow up with IJSA and IJSA+ claimants because no follow-up of these claimants was specified in the original demonstration design. D.C. independently chose to follow up with these claimants as a way to extend the demonstration design.

TABLE IV.2

ESTIMATED OPERATIONAL TREATMENT COSTS PER CLAIMANT
(In Dollars)

	District of Columbia			Florida			New Jersey Structured JSA Treatment
	SJSA	IJSA	IJSA +	SJSA	IJSA	IJSA +	
Total Costs	286	199	216	241	97	103	169
Local Office Costs	251	164	181	205	61	67	144
Labor Costs	209	137	155	136	41	46	118
Local Office Staff	209	137	150	136	41	42	NA
EDWAA Staff	0	0	5	0	0	4	NA
Non-Labor Costs	42	27	29	69	20	21	26
Central Office Costs	35	35	35	36	36	36	25

about 37 percent higher in D.C. This was expected because the Florida group services included fewer participants, so the costs of SJSA, which emphasized group services, were spread over a smaller group in Florida than in D.C. This outcome translates directly into higher relative per- claimant costs in Florida than in D.C.

The largest cost component of any treatment was associated with local office staff. In D.C., the cost for local office staff to provide SJSA was $209 per claimant, which was 73 percent of the total SJSA costs. Similarly, in Florida, the cost for local office staff to provide structured JSA was $136, 56 percent of the total SJSA costs. Nonlabor costs associated with local operations ranged from $20 per claimant for IJSA to $69 for SJSA in Florida. In D.C. the range was from $27 for IJSA to $42 for SJSA.

Costs associated with the central office in each state were a substantial proportion of total costs. In allocating the costs to treatments, we assumed that central office costs per claimant were equal among the three treatments. In D.C., the estimated central office costs were $35 per claimant, which was derived using the generic rate (18 percent) that D.C. applies to direct costs to estimate central office costs in its own accounting system.[2] In contrast, the estimates of Florida central office costs were based on central office staff time and other expenditures that were charged directly to the demonstration. Estimated central office costs in Florida were $36 per claimant, which is nearly identical to D.C. For SJSA, the central office costs represent 12 percent of total costs in D.C. and 15 percent of total costs in Florida. The central office costs represent a larger proportion of the individualized treatments--18 and 16 percent for IJSA and IJSA+ in D.C. and 37 and 35 percent for the IJSA and IJSA+ in Florida.

The data presented in Table IV.2 also allow us to compare the costs of SJSA in D.C. and Florida with the costs of the similar structured JSA treatment in the New Jersey Unemployment Insurance Re-

[2]In D.C. we used the generic rate to estimate the central office costs for the entire demonstration and then spread these costs evenly among treatment group members. Hence, even though direct costs are different between the three treatments, the central office costs are the same.

employment Demonstration. The costs of structured JSA in New Jersey were $169 per claimant, which is substantially lower than the costs of SJSA in either D.C. ($286 per claimant) or Florida ($241 per claimant). A key factor contributing to this difference was inflation of staff compensation costs between 1986-87, when the New Jersey demonstration operated, and 1995-96, when the D.C. and Florida demonstrations operated. Using the increase in the employment cost index for state and local workers, we can translate the New Jersey demonstration costs into 1995-96 dollars. The cost of the New Jersey demonstration in 1995-96 dollars was $246 per claimant, which is very close to the Florida cost estimate and somewhat less than the D.C. estimate.[3] This suggests that inflation of compensation costs accounts for most of the difference between the New Jersey cost estimates and the D.C. and Florida cost estimates.

Several other factors may contribute to structured JSA costs being higher in Florida and D.C. than in New Jersey. For example, D.C. staff tend be more highly paid than New Jersey staff, which at least partly reflects the high cost of living in D.C. Another factor potentially contributing to higher costs in Florida relative to New Jersey is the relatively small number of participants in group services in Florida, which tends to increase costs per claimant.

C. SENSITIVITY OF THE COST ESTIMATES TO ALTERNATIVE ASSUMPTIONS

The cost estimates described above are based on applying a series of assumptions to the actual demonstration charges. There is considerable uncertainty associated with the use of these estimates

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[3]The inflation of the New Jersey costs is based on the seasonally adjusted employment cost index based on total compensation for state and local workers. We use the values for December 1986 and December 1995, which are 89.0 and 129.3.

to evaluate the costs of an ongoing JSA program. First, some of the assumptions, while necessary to generate the estimates, may not accurately reflect reality. For example, we assumed that per- claimant costs in D.C. were similar in the pilot phase and the full demonstration phase, but we have some basis to doubt that assumption. Second, some of the demonstration charges are likely to be associated specifically with the establishment and monitoring of the demonstration and may not accurately reflect costs that would be encountered in an ongoing program. For example, time spent by central office staff in training might be shorter or nonexistent in an ongoing program.

To address this uncertainty and evaluate the effects of different factors on our cost estimates, we generated alternative sets of estimates based on changes in the assumptions underlying the calculations. The new estimates, which are presented in Table IV.3, demonstrate the degree to which the cost estimates are sensitive to alternative assumptions.

The first set of new estimates (alternative estimates 1 in Table IV.3) are based on an alternative treatment of the costs of the D.C. pilot phase. In calculating the baseline estimate of costs per claimant, we simply divided the combined pilot and full demonstration costs by the number of claimants in the pilot and full phases. The calculation implicitly assumes that costs per claimant were the same in the pilot phase as in the full demonstration. But, since the office was fully staffed during the pilot phase even though fewer claimants were served, an alternative assumption would be to assume that costs per week were the same between the two phases, despite the lower level of activity in the pilot phase. Accordingly, we generated alternative estimates of the full phase costs by dividing four-fifths of the total demonstration charges by the number of claimants in the full

TABLE IV.3

ALTERNATIVE ESTIMATES OF OPERATIONAL TREATMENT COSTS PER CLAIMANT

	District of Columbia			Florida		
	SJSA	IJSA	IJSA +	SJSA	IJSA	IJSA +
Baseline Estimates (from Table V.2)	286	199	216	241	97	103
Alternative Estimates						
1. Alternative treatment of D.C. pilot costs	250	174	190	NA	NA	NA
Percent reduction from baseline	-11.2	-10.6	-11.6	NA	NA	NA
2. Central office supervision reduced by 50 percent	268	181	198	223	79	85
Percent reduction from baseline	-6.3	-9.0	-9.1	-7.5	-18.6	-17.5
3. Double the scale of Florida offices	NA	NA	NA	132	74	78
Percent reduction from baseline	NA	NA	NA	-45.2	-23.7	-24.3
4. Exclude small Florida offices from calculation	NA	NA	NA	231	93	99
Percent reduction from baseline	NA	NA	NA	-4.1	-4.1	-3.9
Combined Alternative Estimates[a]	232	156	172	213	75	81
Percent Reduction From Baseline	-18.9	-21.6	-20.4	-11.6	-22.7	-21.4

[a]The combined alternative estimate for D.C. combines alternatives 1 and 2. The combined alternative estimate for Florida combines alternatives 2 and 4.

phase.[4] This treatment of pilot costs reduces the estimate of the D.C. demonstration costs per claimant by about 11 percent, as shown in Table IV.3.

The estimates presented in Table IV.2 suggest that the central office costs represent a substantial proportion of the demonstration costs in Florida and D.C. But in an ongoing program, the central office staff are likely to spend less time managing the program in a typical year than was spent managing a new demonstration. To investigate the impact of accounting for the possibility of lower central office costs, we generated alternative cost estimates (alternative estimates 2 in Table IV.3) based on a 50 percent reduction in the estimated central office costs.

The impact of reducing the central office costs is to generate a modest reduction in the operational demonstration costs. The impact of this reduction is to reduce the total costs of the Florida treatments by 7.5 percent for SJSA, 18.6 percent for IJSA, and 17.5 percent for IJSA+. A similar adjustment in the D.C. cost figures generates similar findings--overall operational cost estimates are reduced by 6 to 9 percent.

Alternative estimates 3 and 4 presented in Table IV.3 are intended to gauge the extent to which the Florida costs could be reduced in an ongoing program by increasing the scale of local operations. This issue arises because the group services, including orientation, testing, and the job search workshop, tended to include small numbers of claimants, so that these group services were relatively costly on a per-claimant basis in Florida. We used two methods to estimate how per-claimant costs would be reduced by increasing the scale of the group services. Our first method was to assume attendance at group services was double the actual attendance and then recalculate the cost estimates based on this assumption. Table IV.3 shows that this assumption generates large reductions in the cost estimates. Since SJSA was the most heavily

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[4]We used four-fifths of the demonstration charges because the pilot phase operated for one calendar quarter compared with four calendar quarters of the full demonstration phase. Hence, if costs per week were similar between the phases, the costs associated with the full phase would represent four-fifths of the total costs.

dependent on group services, the largest cost reduction, which is equal to 45 percent of the baseline estimate, occurs for this treatment. Doubling group service attendance generates smaller but still substantial reductions in IJSA and IJSA+ costs of 24 percent.

A second method we used to address this issue is to exclude the smallest Florida offices from the cost calculations. When we examined demonstration costs by office, we found that per-claimant costs varied widely. Local demonstration costs ranged from $86 per claimant to $253 per claimant, depending on the office. But we found no clear relationship between the per-claimant costs in a given office and the number of demonstration participants in that office. Hence, excluding the small offices from the cost calculations had only a modest effect on estimated per-claimant costs. As shown in Table IV.3, excluding the three smallest demonstration offices from the calculations yielded cost figures that were only about 4 percent lower than the baseline estimates.[5]

Our final set of estimates is based on combining alternative estimates. For D.C., we combined the change in the pilot cost treatment and the 50-percent reduction in central office costs to yield cost estimates that are roughly 20 percent less than the baseline estimates. For Florida, we decided that achieving savings by doubling the scale of the Florida offices is probably unrealistic, especially given that we did not find a strong relationship between office caseloads and per claimant costs. Hence, we ignored alternative 3 and instead, combined alternatives 2 and 4--the estimates based on a 50 percent reduction in central office costs and those that exclude the smallest offices from the cost calculations. Combining these alternatives yields Florida cost estimates that are 12 to 23 percent less than the baseline estimates.

We consider the combined alternative estimates to represent a rough lower-bound of the costs of an ongoing program. At the same time, we consider the baseline estimates to be upper-bound estimates, since it is hard to envision factors that would make an ongoing program more expensive than the demonstration.

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[5] In these calculations we excluded the Pensacola, St. Augustine, and Clearwater offices. These three offices had the fewest claimants assigned to each of the JSA group services.

The costs of an ongoing program therefore are probably between the baseline estimates and the estimates on the final line in Table IV.3.

V. IMPACTS ON UNEMPLOYMENT INSURANCE RECEIPT

It was expected that by encouraging workers to search more effectively and aggressively for a new job, the JSA demonstration services would speed re-employment and reduce the UI benefits claimed by the demonstration participants. Our analysis of the impacts on UI outcomes was intended to determine whether the JSA treatments reduced the level of UI receipt and, if so, the extent of the reduction. Our approach to estimating the impacts on UI receipt was to compare UI receipt among claimants in the treatment groups with UI receipt among claimants in the control group. Because claimants were randomly assigned to the treatment and control groups, the differences in outcomes between the treatment and control groups provide unbiased estimates of the treatment impacts.

In this chapter, we address three types of outcomes related to UI receipt. In Section A, we present estimated impacts of the JSA treatments on broad measures of average UI benefit receipt in the initial benefit year (year 1) and in the following year (year 2), including weeks of UI benefits, dollars of UI benefits, and whether claimants exhausted their benefit entitlements. In addition to the impacts on average UI receipt, we also estimated the impact of the treatments on the overall distribution of weeks on UI in our analysis of UI exit rates. This analysis helped us to identify those weeks during a UI spell in which the impacts of the JSA treatments typically occur. Finally, we expected that the treatments might have greater impacts on particular subgroups of claimants, such as those with the highest predicted probabilities of benefit exhaustion. As documented in Section C, we estimated the impacts of the treatments for various claimant subgroups.

The estimates presented in this chapter demonstrate that the JSA treatments reduced UI receipt significantly over the initial benefit year. The largest impact occurred in D.C. for the SJSA treatment, which reduced average UI receipt by more than a week, or by $182 per claimant. The other five JSA treatments across the two states had more modest impacts, reducing average UI receipt by about half

a week, or by $100 or less. Most of the treatments also significantly reduced the proportion of claimants who exhausted their benefits in the initial benefit year. In the longer run, however, none of the treatments had an impact on new claims or on the amount of UI benefits received beyond the initial benefit year.

The investigation of the UI exit rates revealed two important findings. First, at least some of the treatments reduced UI spells in the initial benefit year among claimants who, in the absence of the treatments, would have had relatively long UI spells and were likely to exhaust their benefits. Second, the impacts on UI exit rates occurred early in the UI spells, around the time claimants were notified of the JSA service requirements or would have been scheduled to participate in services. This finding suggests that much of the impact on UI receipt results from an immediate response by claimants to the services.

Our subgroup analysis of impacts in the initial benefit year was designed to provide information about the extent to which demonstration services were targeted to those most likely to be affected by those services, and about the extent to which impacts on UI outcomes vary across different claimant subpopulations. The results suggest that even if D.C. and Florida had selected even more restrictive eligibility criteria, i.e. higher probability thresholds that excluded either the bottom quarter or half of those actually deemed eligible, the average impact of being assigned to one of the JSA treatments would have been largely unaffected. Furthermore, in D.C., the impacts of these treatments on UI benefits were particularly large for young people (all three treatments) and for whites (IJSA and IJSA+). In Florida, the impacts of the treatments were particularly large for women (IJSA and IJSA+) and those previously employed in trade industries (SJSA).

A. UNEMPLOYMENT INSURANCE RECEIPT

The primary outcome of interest in evaluating the JSA demonstration is UI benefit receipt. For most of our analysis, we used three measures to examine UI receipt among the demonstration

participants: (1) the numbers of weeks for which each claimant was paid benefits in the benefit year, (2) the dollar amount of UI benefits paid to claimants in the benefit year, and (3) whether claimants exhausted their benefits.

1. Average Benefit Receipt in the Control Group

The control group provides a basis for evaluating what would have happened to the target population without the demonstration. Average UI receipt in the initial benefit year among the control groups in each state is shown in Table V.1. A comparison of UI receipt in the two states shows that the control group in D.C. received greater UI benefits during the initial benefit year than did the control group in Florida. Control group claimants in D.C. received 20.1 weeks of benefits, on average, compared with 15.8 weeks for claimants in Florida. Total benefit receipt in dollars was $4,236 in D.C. compared with $2,728 in Florida, a difference of more than 50 percent. The degree to which claimants exhausted their benefits was also much higher in D.C.--about 59 percent of D.C. claimants exhausted benefits compared with 45 percent of Florida claimants.

The differences in the average UI measures in the two states arose primarily because average UI entitlements were higher in D.C. than in Florida. The average benefit entitlement in dollars for control group members was $5,463 in D.C. compared with $4,056 in Florida (see Chapter II, Table II.3). This difference in UI entitlement arose primarily from the differences in average weekly benefit amounts in the two states, which was caused partly by the difference in each state's statutory maximum weekly benefit. In D.C., the maximum weekly benefit amount was set at $347 until January 1996, when it was increased to $359. This was considerably higher than Florida's

TABLE V.1

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE ON UI OUTCOMES IN THE INITIAL BENEFIT YEAR (YEAR 1)
(Standard Errors in Parentheses)

	District of Columbia				Florida			
Outcome	Control Group Mean	SJSA Group Mean	Estimated Impact	Estimated Impact as a Percentage of the Control Group Mean	Control Group Mean	SJSA Group Mean	Estimated Impact	Estimated Impact as a Percentage of the Control Group Mean
Weeks of UI Benefits Received in Benefit Year	20.14 (0.19)	19.01 (0.21)	-1.13*** (0.29)	-5.6	15.81 (0.16)	15.40 (0.17)	-0.41** (0.23)	-2.6
Dollars of UI Benefits Received in Benefit Year	4,236 (58)	4,053 (61)	-182** (84)	-4.3	2,728 (36)	2,689 (37)	-39 (52)	-1.4
Rate of UI Benefit Exhaustion (Percent)	58.8 (1.1)	53.9 (1.1)	-4.8*** (1.6)	-8.2	45.0 (0.9)	43.2 (0.9)	-1.8* (1.3)	-4.0
Sample Size	2,012	2,026			3,014	3,032		

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in mean outcomes between the control group and SJSA group.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

maximum weekly benefit amount which was set at $250 during the demonstration. In terms of actual benefit amounts, claimants in the D.C. control group received $212 per week on average, compared with $175 per week for those in the Florida control group.

D.C. claimants could also, on average, receive benefits for a longer maximum period—they were all entitled to 26 weeks of benefits. Most Florida claimants were also entitled to 26 weeks of benefits, but some were entitled to as few as 10 weeks of benefits. Across all Florida control group members, average potential duration was 22.7 weeks.

One implication of the cross-state differences in control group UI receipt is that D.C. might have had greater potential than Florida for UI benefit reductions. For example, a 5 percent reduction in the length of UI spells would have translated into greater UI savings in D.C. because the D.C. control group received more benefits than the Florida group. Even a one-week reduction in UI spells would likely have generated greater savings in D.C., since the average weekly benefit amount was higher in D.C. than in Florida.[1]

2. Estimated Impacts on UI Receipt

We estimated the impact of the JSA treatments on UI receipt by comparing average UI receipt for the control group with that for the treatment groups. Since claimants were assigned randomly to these groups, the differences in average UI receipt are unbiased estimates of the impact of the treatments on average UI receipt. Alternatively, we could have presented regression estimates of the treatment impacts. The use of regressions would have allowed us to control for variation in personal characteristics in isolating the impacts on UI receipt. However, we tested this alternative and found

[1]This is not necessarily the case, since claimants responding to the treatments in the two states may have had different average weekly benefit amounts.

that regression estimates were nearly identical to estimates based on treatment-control differences. We therefore present only the treatment-control differences of means.

We present average UI outcomes and estimated impacts for both year 1--the initial benefit year--and year 2--the year following the initial benefit year. The year 2 UI outcomes include all UI receipt resulting from an initial claim filed within one year of the end of the initial benefit year.

a. Structured JSA

Our estimates demonstrate that SJSA clearly reduced average UI receipt in the D.C. demonstration. As shown in Table V.1, SJSA reduced UI receipt in the initial benefit year by 1.13 weeks, which represents 5.6 percent of the average UI weeks paid to the control group. In dollar terms, SJSA reduced UI receipt by $182 per claimant. SJSA also reduced the rate at which claimants exhausted their UI benefits--the exhaustion rate was nearly 5 percentage points lower for the treatment group than for the control group. All of these estimates are statistically significant at least at the 95 percent confidence level.

The estimates for Florida provide somewhat weaker evidence that SJSA reduced UI receipt in that state. In Florida, the estimated impact of SJSA on the length of the average UI spell was only -0.41 weeks, which represents 2.6 percent of the control group mean. Similarly, the estimated benefit reduction in dollar terms was only $39 per claimant, while the estimated impact on benefit exhaustion was a modest 1.8 percentage point reduction. The estimated impacts on UI weeks and benefit exhaustion are statistically significant at the 95 and 90 percent confidence levels, respectively.

Although the estimated impacts in Florida are small relative to those in D.C., they are broadly consistent with the findings from the New Jersey UI Re-employment Demonstration. In New Jersey, we estimated that a structured JSA package reduced the duration of benefit receipt by 0.47 weeks (compared with 0.41 weeks in Florida). The estimated reductions in dollars of benefits and probability

of exhaustion were somewhat higher in New Jersey ($87 and 2.8 percentage points, respectively) than in Florida, although estimates from both states suggest modest reductions in UI receipt.

The differences in the impacts of SJSA on UI in D.C. and in Florida were probably due to a variety of factors, but one potential contributor was D.C.'s relative aggressiveness in enforcing the demonstration participation requirements.[2] As explained in previous reports on the JSA demonstration, claimants in D.C. who did not comply with the JSA service requirements (no-shows) were not sent a benefit check. The no-shows were then expected to report to a UI claims examiner in the demonstration office and reschedule their service before they could receive their checks. In contrast, Florida did not automatically interrupt the payment of benefits to no-shows. Florida claimants who missed a service could continue to receive their benefits and reschedule missed services over the telephone rather than reporting to the demonstration office in person. Only after a Florida claimant failed to show for the same service three times was the next benefit claim rejected.

The more rigorous enforcement of demonstration participation requirements in D.C. could have had two effects. First, enforcing the D.C. requirements may have increased the service participation rate, which could have increased the effectiveness of the job search activities among the target group, thereby generating more rapid re-employment. Findings presented in Chapter IV confirm that among the SJSA claimants, the orientation and job search workshop attendance rates were higher in D.C. than in Florida. Second, enforcing the D.C. requirements may have caused claims to be scrutinized more closely so that the D.C. no-shows were denied benefits more often than Florida no-shows.[3] Either of these potential effects would have reduced UI receipt--in one case because claimants would have gone

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[2] Another potential contributor is the higher control group receipt in D.C. compared with Florida, which was mentioned in the previous section. The higher control group receipt in D.C. may have provided greater potential for UI benefit reductions in D.C. than in Florida.

[3] We examine benefit denial rates in Chapter VI.

back to work more quickly, and in the other case because claimants would have been denied benefits more readily.

In both of the JSA demonstration states, the estimated impacts on UI dollars were smaller than might have been expected given the impacts on weeks. However, this difference can be explained by results suggesting that the impacts of JSA treatments on weeks of UI receipt were larger for claimants with small weekly benefit amounts. If the impact of each JSA treatment were independent of the weekly benefit amount,[4] then we should be able to impute the average impact on UI dollars by multiplying the average impact on weeks by the average weekly benefit amount. The imputed impacts of SJSA based on this method are a $240 benefit reduction in D.C. and a $72 reduction in Florida. These imputed impacts exceed the direct estimates of the impacts on UI dollars that are shown in Table V.1 ($182 in D.C. and $39 in Florida). This discrepancy suggests that the impact of SJSA on weeks paid was larger for claimants with low monthly benefit amounts. According to our calculations in D.C., SJSA reduced weeks paid by two weeks for claimants in the lowest quartile of the weekly benefit amount, by slightly more than one week for claimants in the middle two quartiles, and by nothing for claimants in the top quartile.[5] As a result, the reduction in the number of weeks for which benefits

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[4]In the simplest case, suppose that the impact of each treatment is identical for all claimants, regardless of their weekly benefit amounts.

[5]Allowing the impact to vary with the weekly benefit amount , we again imputed the impact of SJSA on UI dollars from the estimated impacts on weeks paid. For each quartile of the weekly benefit amount, we multiplied the impact on weeks paid by the average weekly benefit amount in the quartile to impute the impact on UI dollars for that quartile. We then averaged the imputed impacts across all four quartiles to impute an average reduction in UI payments of $175. This imputed impact is very close to the direct estimate of -$182.

were paid led to a reduction in UI payments that was smaller than it would have been had the impact been unrelated to the weekly benefit amount.

The SJSA treatment did not have an effect on UI receipt beyond the initial benefit year. Table V.2 presents the estimated impacts of SJSA on measures of UI receipt in year 2. SJSA had no statistically significant impact on any of the year 2 UI measures in either D.C. or Florida, and the point estimates are consistently small. For example, in D.C., the rate of new claim filing in year 2 was nearly identical for the SJSA and the control groups--17.1 percent for the control group and 17.0 percent for the SJSA group--implying that the SJSA treatment had no substantial impact on new claims. The rate at which new claims were filed in year 2 was somewhat lower in Florida than in D.C., but the rates were similar for the Florida SJSA and control groups--around 14 percent for both groups--further confirming the SJSA had no impact on new UI claims. The rates of average weeks of UI receipt, dollars of UI receipt, and benefit exhaustion were also similar across the SJSA and control groups in both states.

Our finding that SJSA had no impact on UI receipt in year 2 differs from findings from the New Jersey UI Re-employment Demonstration (Corson and Haimson 1996). In that demonstration, researchers found that a structured JSA package significantly reduced UI receipt in the second year after the initial claim as well as in the initial benefit year. Based on this finding, the researchers speculated that the JSA package induced participants to find jobs that were, on average, more stable than jobs found by the control group. If the SJSA treatment in the JSA Demonstration generated similar long-run job stability in D.C. or Florida, it is apparently not reflected in reduced year 2 UI receipt. Although this finding is not consistent with the findings from the New Jersey demonstration, it is consistent with our expectations--we did not expect that SJSA would induce

TABLE V.2

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE ON UI OUTCOMES IN YEAR 2
(Standard Errors in Parentheses)

	District of Columbia			Florida		
Outcome	Control Group Mean	SJSA Group Mean	Estimated Impact	Control Group Mean	SJSA Group Mean	Estimated Impact
Rate of New Claims Filing (Percent)	17.1 (0.8)	17.0 (0.8)	-0.1 (1.2)	13.9 (0.6)	14.5 (0.6)	0.6 (0.9)
Weeks of UI Benefits Received in Benefit Year	3.13 (0.17)	3.26 (0.18)	0.12 (0.25)	1.79 (0.09)	1.90 (0.10)	0.11 (0.14)
Dollars of UI Benefits Received in Benefit Year	571 (35)	614 (37)	43 (51)	321 (18)	338 (19)	17 (26)
Rate of UI Benefit Exhaustion (Percent)	7.8 (0.6)	8.2 (0.6)	0.4 (0.9)	6.4 (0.4)	7.0 (0.5)	0.5 (0.6)
Sample Size	2,012	2,026		3,014	3,032	

SOURCE: State UI claims records.

NOTE: The year 2 UI outcomes include all UI receipt resulting from an initial claim filed within one year of the end of the benefit year used to define year 1 UI receipt. The impact estimates are based on differences in mean outcomes between the control group and SJSA group.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

claimants to take jobs that differed substantially from those held by the control group. We re-examine the issue of job stability in the Chapter VII, in which we present estimated impacts on employment and earnings in year 2.

b. Individualized JSA

The impacts of the two individualized JSA treatments (IJSA and IJSA+) on UI receipt were estimated just as the impact of SJSA was estimated--by comparing average outcomes of the treatment groups with those of the control group. In this section, we present estimates of the impacts of IJSA and of IJSA+. However, our analysis of service participation in Chapter IV suggests that there was little distinction between the two treatments with respect to the actual services received, and we therefore expected the treatments to have similar impacts on UI receipt. The estimates presented in this section are consistent with this expectation.

Across the two states, IJSA and IJSA+ appear to have had modest impacts on UI receipt. In D.C., the treatments reduced average UI receipt by about half a week--0.47 weeks for IJSA and 0.61 weeks for IJSA+ (Table V.3). Both of these impacts are statistically significant at the 95 percent confidence level. The IJSA and IJSA+ groups also received less than the control group in UI dollars, but the differences are small and not statistically significant. IJSA+ reduced the rate of benefit exhaustion by an estimated 3.9 percentage points. The corresponding estimate for IJSA shows that IJSA also reduced UI exhaustion. The point estimate for the IJSA impact (2.4 percentage points) is smaller than the IJSA+ estimate, but the difference is not statistically significant.

The individualized treatments had similarly modest impacts on UI receipt in Florida. As shown in Table V.3, both IJSA and IJSA+ reduced UI receipt by about half a week, and these estimates are statistically significant at least at the 95 percent confidence level. IJSA also reduced UI dollars receipt by $100 and UI exhaustion by 2.4 percentage points. IJSA+ had broadly similar impacts,

TABLE V.3

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE TREATMENTS ON UI OUTCOMES IN THE INITIAL BENEFIT YEAR (YEAR 1)
(Standard Errors in Parentheses)

Outcome	Control Group Outcome	IJSA			IJSA +		
		Treatment Group Outcome	Estimated Impact	Estimated Impact as a Percentage of the Control Group Mean	Treatment Group Outcome	Estimated Impact	Estimated Impact as a Percentage of the Control Group Mean
DISTRICT OF COLUMBIA							
Weeks of UI Benefits Received in Benefit Year	20.14 (0.19)	19.68 (0.20)	-0.47** (0.28)	-2.3	19.54 (0.21)	-0.61** (0.28)	-3.0
Dollars of UI Benefits Received in Benefit Year	4,236 (58)	4,180 (60)	-56 (84)	-1.3	4,198 (60)	-37 (84)	-0.9
Rate of UI Benefits Exhaustion (Percent)	58.8 (1.1)	56.4 (1.1)	-2.4* (1.6)	-4.1	54.9 (1.1)	-3.9*** (1.6)	-6.6
Sample Size	2,012	2,022			2,011		
FLORIDA							
Weeks of UI Benefits Received in Benefit Year	15.81 (0.16)	15.21 (0.16)	-0.59*** (0.23)	-3.7	15.29 (0.16)	-0.52** (0.23)	-3.3
Dollars of UI Benefits Received in Benefit Year	2,728 (36)	2,628 (36)	-100** (51)	-3.7	2,655 (36)	-73* (51)	-2.7
Rate of UI Benefit Exhaustion (Percent)	45.0 (0.9)	42.6 (0.9)	-2.4** (1.3)	-5.3	42.2 (0.9)	-2.8** (1.3)	-6.2
Sample Size	3,014	3,007			2,989		

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the IJSA and IJSA+ groups.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

reducing UI dollars by an estimated $73 and benefit exhaustion by 2.8 percentage points. It is somewhat surprising that the point estimates for IJSA and IJSA+ in Florida are larger than the SJSA estimate, although the differences are not statistically significant. Our expectation was that SJSA impacts would be larger than the IJSA and IJSA+ impacts, which was the case in D.C.

Neither of the individualized treatments had impacts on UI receipt in the longer run. Like SJSA impacts, the IJSA and IJSA+ impacts on UI receipt in year 2 were typically small and statistically insignificant. For example, Table V.4 shows that in D.C., the rate at which new claims were filed was slightly lower for the IJSA and IJSA+ groups than for the control group, but neither of the differences is significantly different from zero. In Florida, the rate at which new claims were filed was slightly higher for the IJSA and IJSA+ groups, but again the differences are not statistically significant. Given the modest impacts of IJSA and IJSA+ on UI receipt in year 1, we are not surprised to find no substantial impacts in year 2.

If we compare the estimated IJSA and IJSA+ impacts with the estimated SJSA impacts in the two states, we cannot definitively conclude that one service strategy was more effective than the others in reducing the duration of UI spells. In D.C., the SJSA impact in year 1 was about half a week larger than the average IJSA and IJSA+ impacts, and the difference is statistically significant. But in Florida, the SJSA impact in year 1 was nearly identical to the IJSA and IJSA+ impacts. In year 2, none of the treatments had a significant impact on UI receipt in either state.

The differences (or lack of differences) between the impact estimates for IJSA and IJSA+ are not indicative of the impact of training on demonstration claimants. We noted in Chapter IV that the rate of training participation was no higher in the IJSA+ group than in the IJSA group. Given this finding, it is not surprising that the impacts on the two groups were similar. Any differences in

TABLE V.4

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE TREATMENTS ON UI OUTCOMES IN YEAR 2
(Standard Errors in Parentheses)

Outcome	Control Group Mean	IJSA Treatment Group Mean	IJSA Estimated Impact	IJSA + Treatment Group Mean	IJSA + Estimated Impact
DISTRICT OF COLUMBIA					
Rate of New Claim Filing (Percent)	17.1 (0.8)	15.8 (0.8)	-1.3 (1.2)	16.8 (0.8)	-0.3 (1.2)
Weeks of UI Benefits Received in Benefit Year	3.13 (0.17)	2.98 (0.17)	-0.15 (0.24)	3.08 (0.17)	-0.06 (0.25)
Dollars of UI Benefits Received in Benefit Year	571 (35)	548 (34)	-23 (49)	586 (36)	15 (50)
Rate of UI Benefits Exhaustion (Percent)	7.8 (0.6)	6.8 (0.6)	-1.0 (0.8)	8.4 (0.6)	0.7 (0.9)
Sample Size	2,012	2,022		2,011	
FLORIDA					
Rate of New Claim Filing (Percent)	13.9 (0.6)	14.0 (0.6)	.01 (0.9)	14.9 (0.7)	0.9 (0.9)
Weeks of UI Benefits Received in Benefit Year	1.79 (0.09)	1.82 (0.09)	0.03 (0.13)	1.96 (0.10)	0.17 (0.14)
Dollars of UI Benefits Received in Benefit Year	321 (18)	323 (18)	2 (26)	348 (19)	27 (26)
Rate of UI Benefit Exhaustion (Percent)	6.4 (0.4)	7.2 (0.5)	0.8 (0.7)	7.4 (0.5)	1.0* (0.7)
Sample Size	3,014	3,007		2,989	

SOURCE: State UI claims records.

NOTE: The year 2 UI outcomes include all UI receipt resulting from an initial claim filed within one year of the end of the benefit year used to define year 1 UI receipt. The impact estimates are based on differences in outcomes between the control group and the IJSA and IJSA+ groups.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

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outcomes between the two groups therefore cannot be attributed to training and are more likely due to sampling variation.

B. IMPACTS ON UI EXIT RATES

The previous section provides estimates of the impact of the JSA treatments on mean UI outcomes. Additional information is provided by examining the impact of the JSA treatments on the entire distribution of weeks on UI. In this section, we present for each state the weekly UI exit rates for the control group and the JSA treatment groups, as well as impact estimates based on the difference between these weekly rates.

1. Structured JSA

The weekly cumulative exit rates for the control group and SJSA group for D.C. are shown in Figure V.1. These cumulative rates show the proportion of claimants in each group whose UI spell was equal to or less than the week shown in the figure.[6] The cumulative exit rates for both groups increased steadily over time, demonstrating that D.C. claimants stopped receiving UI at a relatively steady weekly rate. Approximately 40 percent or more of claimants assigned to either group exited UI prior to week 26 of their spell, which is when most claimants would have exhausted their benefits.

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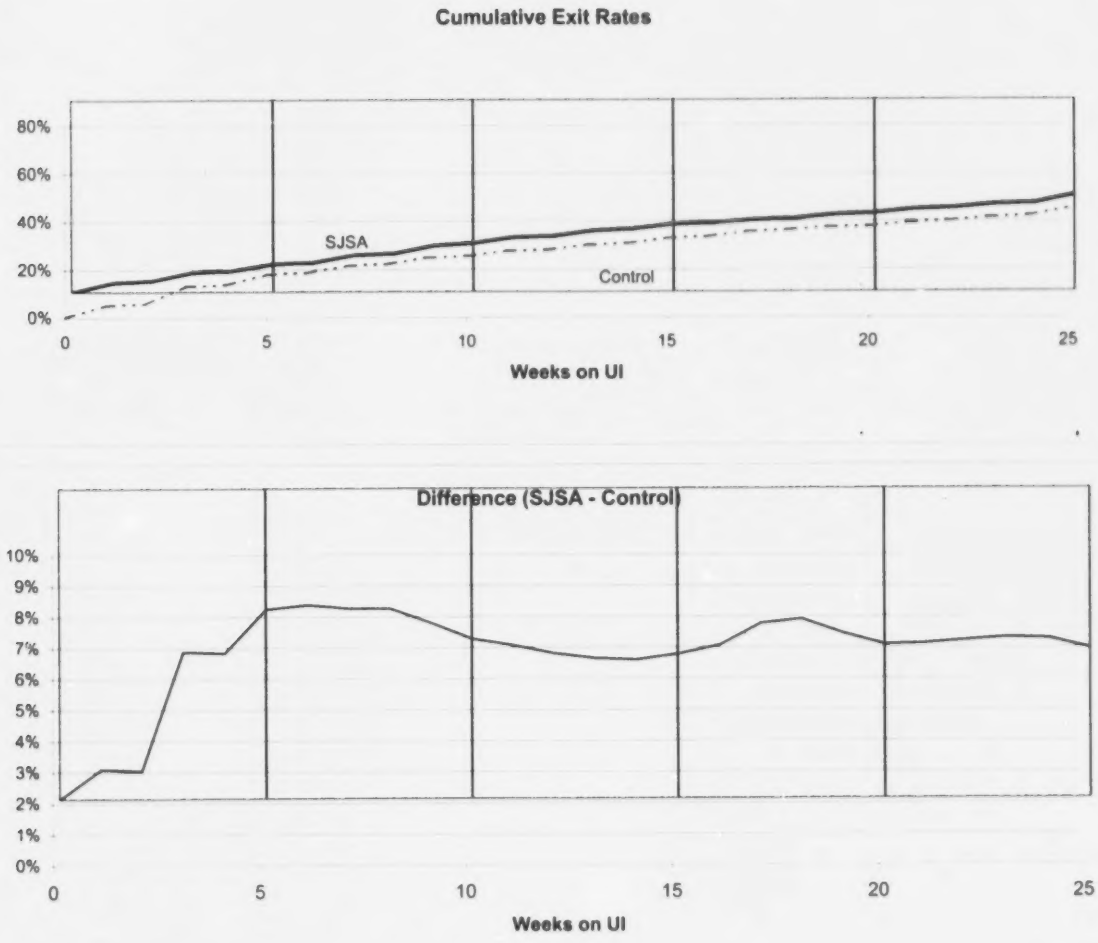
[6]The estimate of the cumulative UI exit rate at week j is based on the following expression:

$$H_j = (1-S_j);$$

where $S_j = ?_{i=1,j}(1-h_j)$ and h_j is the conditional exit rate in week j. The conditional exit rate is equal to the number of claimants who voluntarily end their UI spell during the week divided by the number of claimants still receiving UI at the beginning of the week. The term S_j is the Kaplan-Meier estimate of the survival rate--an estimate of the proportion of the original sample that has not yet exited UI.

FIGURE V.I

IMPACT OF STRUCTURED JSA ON CUMULATIVE UI EXIT RATES, DC



SJSA increased the rate at which D.C. claimants exited UI throughout the entire potential UI spell. The impact of SJSA is represented by the difference between the exit rates for the SJSA and control groups. This difference, which is shown in the lower panel of Figure V.1, reaches a maximum of slightly greater than 6 percentage points in week 6 and then hovers between 4 and 6 percentage points for the remainder of the UI spell. We also conducted significance tests of these differences at five-week intervals, which are shown in Table V.5. At each interval the difference in exit rates between the control group and SJSA group is significant at the 99 percent confidence level.

In addition to being statistically significant, the differences shown in Figure V.1 are substantial. Furthermore, the timing of the impacts suggests that SJSA affected claimants who would otherwise have had long-term UI spells. At the five-week mark, the cumulative exit rate for the SJSA group was 17.7 percent, which was more than 50 percent higher than the 11.6 percent rate for the control group. The absolute magnitude of this difference then remained relatively steady over time, even though the SJSA services were received early in the UI spell. The persistence of this difference suggests that some claimants whose UI receipt was reduced as a function of their response to SJSA would have exhausted their benefits without the treatment.[7] This evidence supports the findings presented in Table V.1, which shows that SJSA reduced the rate of benefit exhaustion in D.C. by 5 percentage points. This finding is also consistent with the New Jersey UI Re-employment Demonstration, in which a similar SJSA treatment significantly reduced the rate of benefit exhaustion.

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[7]Since the SJSA-control difference persists until the point of exhaustion, this implies that some members of the SJSA group who exited UI early in response to SJSA have counterparts in the control group who did not exit UI and therefore exhausted their benefits. We can therefore conclude that some of the early exiters in the SJSA group would have exhausted their benefits in the absence of SJSA services.

TABLE V.5

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE ON UI EXIT RATES

Length of UI Spell, in Weeks	District of Columbia			Florida		
	Cumulative Exit Rates (%)			Cumulative Exit Rates (%)		
	Control Group	SJSA Group	Estimated Impact	Control Group	SJSA Group	Estimated Impact
5	11.6	17.7	6.1***	17.5	21.0	3.5***
10	20.1	25.3	5.1***	30.4	34.9	4.5***
15	27.9	32.6	4.6***	42.7	44.1	1.3
20	32.4	37.4	5.0***	50.5	52.4	1.9*
25	39.9	44.7	4.8***	58.5	59.5	1.0

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in exit rates between the control group and the SJSA group.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

As expected, SJSA had a smaller impact on UI exit rates in Florida than in D.C. The top panel in Figure V.2 shows that, in Florida, about 60 percent of the claimants in both the control group and SJSA groups exited UI by week 26. However, the rate for the SJSA group was higher throughout the observation period. The difference between the groups, which is shown in the lower panel of Figure V.2, reaches a maximum in week 8 of 5 percentage points, which is lower than the maximum of more than 6 percentage points in D.C. Furthermore, the difference between the two groups in Florida slowly becomes smaller after reaching a maximum, so that it hovers between 1 and 2 percentage points beginning in week 14. This decay in the impact of SJSA on the exit rate suggests that the Florida claimants whose UI spells were shortened as a function of their response to the SJSA treatment were unlikely to have had long UI spells even without the demonstration.[8] This finding is consistent with the estimated impact of SJSA on benefit exhaustion in Florida, which was relatively small (a 1.8 percentage point reduction).

Significance tests of the differences at five-week intervals support these findings. As shown in Table V.5, the estimated impact of SJSA on the cumulative UI exit rate in Florida is substantial and significant at weeks 5 and 10. However, this impact decays over time, and at weeks 15 and 25 the estimated impacts are not statistically significant. This pattern differs from the persistent significance of the D.C. impacts shown in the table.

One finding that is the same for the two states is that the impact of SJSA on UI exit rates first appears early in the UI spells. In both states, a substantial proportion of the maximum impact occurred in the first three or four weeks of the UI spell. This impact occurred at approximately the

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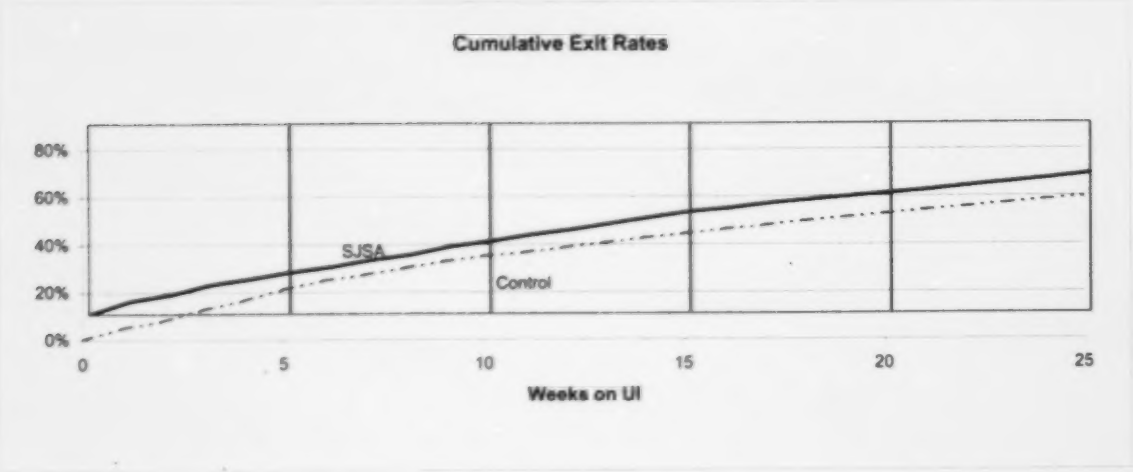
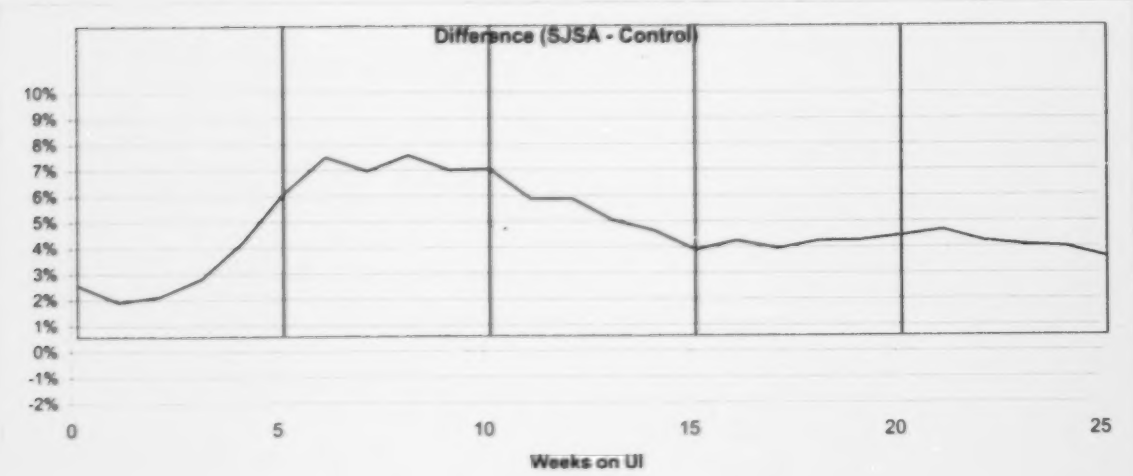
[8]The decay of the SJSA-control difference suggests that while some SJSA claimants exited UI earlier than their counterparts in the control group, the control group counterparts subsequently exited UI by week 15, too early to exhaust their UI benefits. Hence, we conclude that the early exiters in the SJSA group would still have exited by week 15 in the absence of SJSA.

FIGURE V.2

IMPACT OF STRUCTURED JSA ON CUMULATIVE UI EXIT RATES, FLORIDA





same time that claimants were being notified of the JSA services and beginning to participate in them. Other tests of mandatory services provided to UI claimants have yielded similar findings.[9]

The timing of the impacts on UI exit rates implies that much of the impact of SJSA is due to an immediate response to being notified of the services and to participating in them. Claimants assigned to SJSA may have immediately conducted effective job searches using the information they received and techniques they learned through the various services. Similarly, claimants may simply have been encouraged by the JSA services to go out and find a job immediately. Finally, claimants may have returned to work or even simply stopped claiming benefits to avoid the JSA participation requirements. Our findings do not allow us to distinguish between these explanations.

2. Individualized JSA

The timing of the impacts of both individualized JSA treatments was similar to the timing of the SJSA impacts. All treatments achieved their maximum impacts early in what could have been much longer UI spells. In D.C., both individualized treatments achieved their maximum impact in week 6 (Figures V.3 and V.4), while in Florida, both achieved their maximum in week 8 (Figures V.5 and V.6).

However, the impact estimates for IJSA and IJSA+ in both states tend to be modest and are often not statistically significant. In D.C., the estimated IJSA and IJSA+ impacts, which are shown in Table V.6, are clearly smaller than the estimated SJSA impacts shown in Table V.5 For example, the maximum estimated impacts of the IJSA and IJSA+ treatments in D.C. were 3.2 and 4.2

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[9]Corson and Decker (1989) showed that in the New Jersey UI Re-employment Demonstration a significant part of the impact of JSA on UI exit rates occurred at the time that claimants were notified of services or were required to participate in services. Johnson and Klepinger (1994) detected a similar effect of mandatory services in the Washington Alternative Work Search Experiment.

FIGURE V.3

IMPACT OF INDIVIDUALIZED JSA ON CUMULATIVE UI EXIT RATES, DC

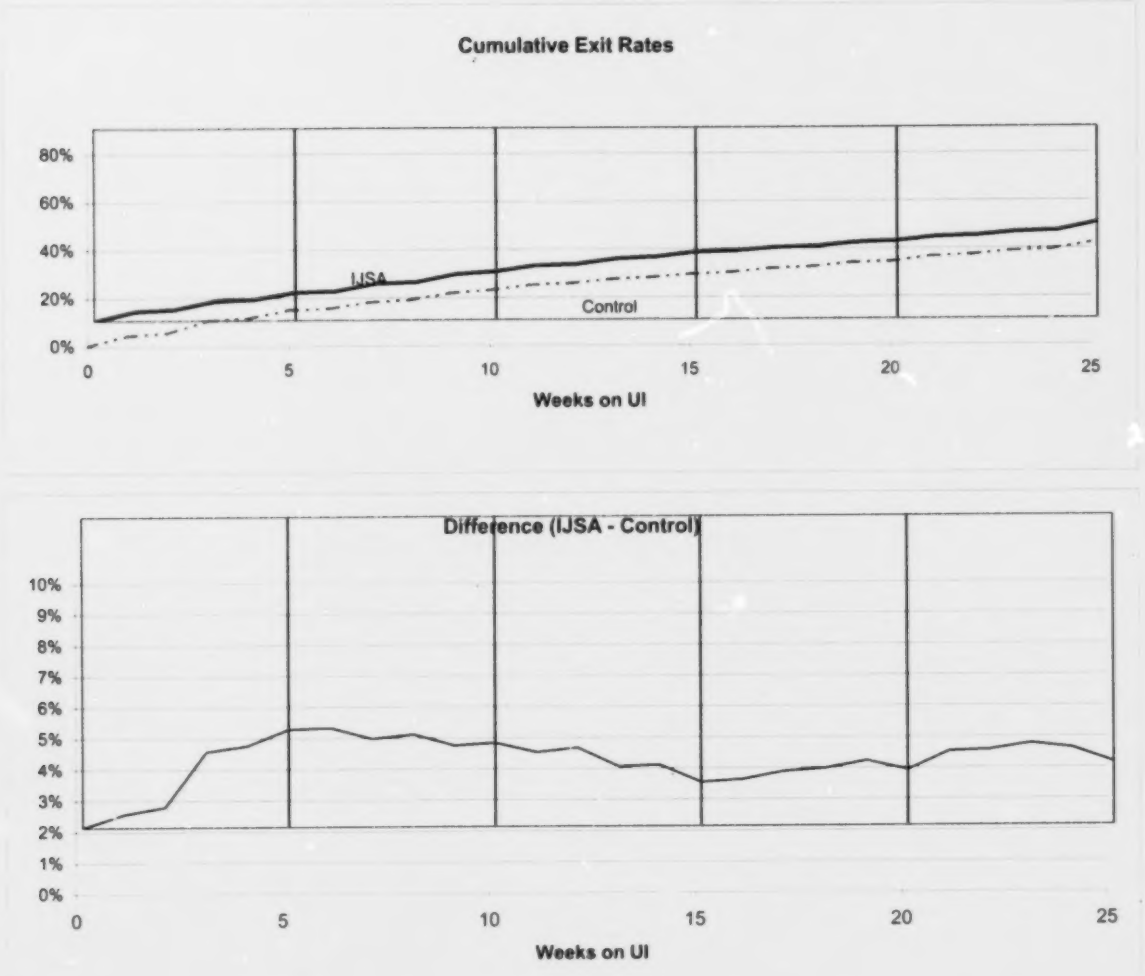

FIGURE V.4

IMPACT OF INDIVIDUALIZED JSA ON
CUMULATIVE UI EXIT RATES,
FLORIDA

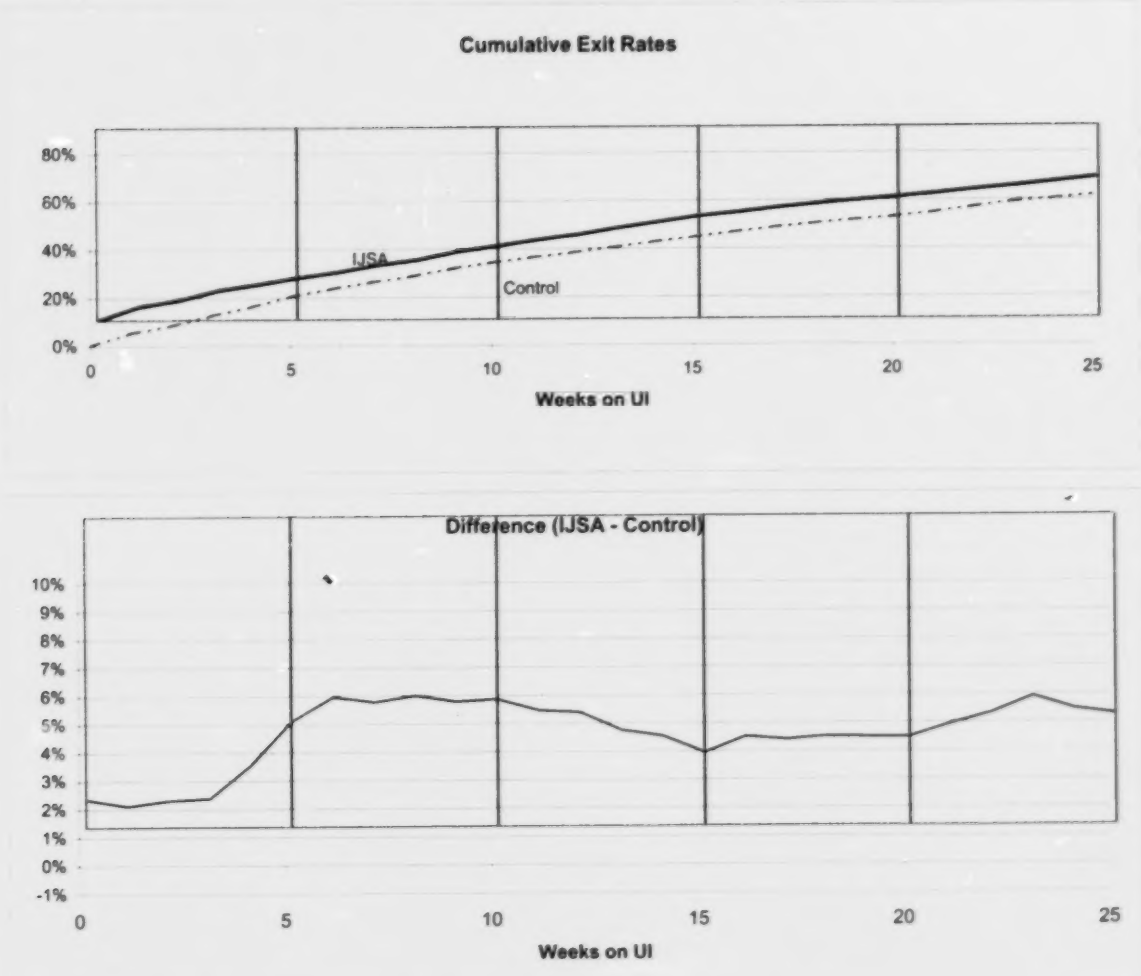

FIGURE V.5

IMPACT OF INDIVIDUALIZED JSA WITH TRAINING ON CUMULATIVE UI EXIT RATES, DC

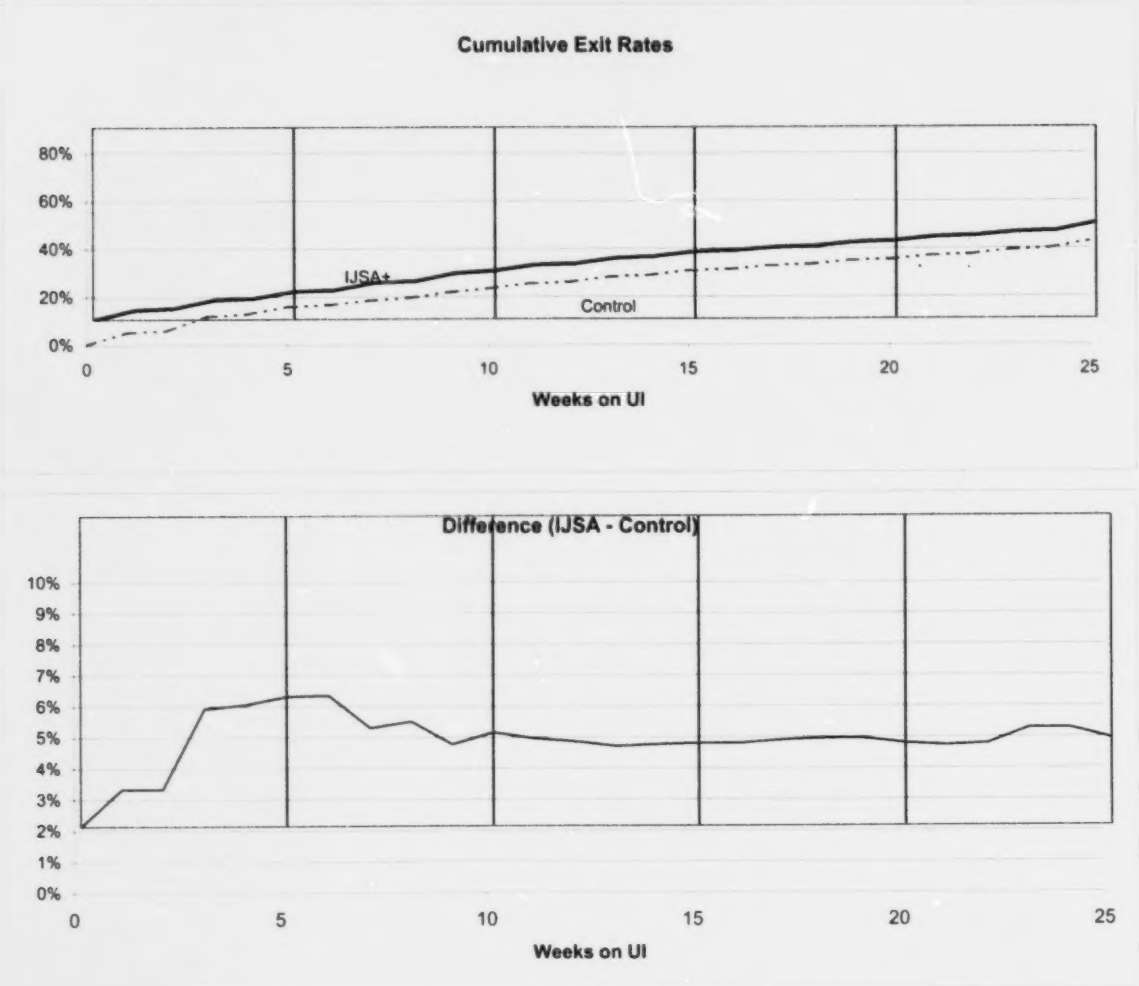

FIGURE V.6

IMPACT OF INDIVIDUALIZED JSA WITH TRAINING ON CUMULATIVE UI EXIT RATES, FLORIDA

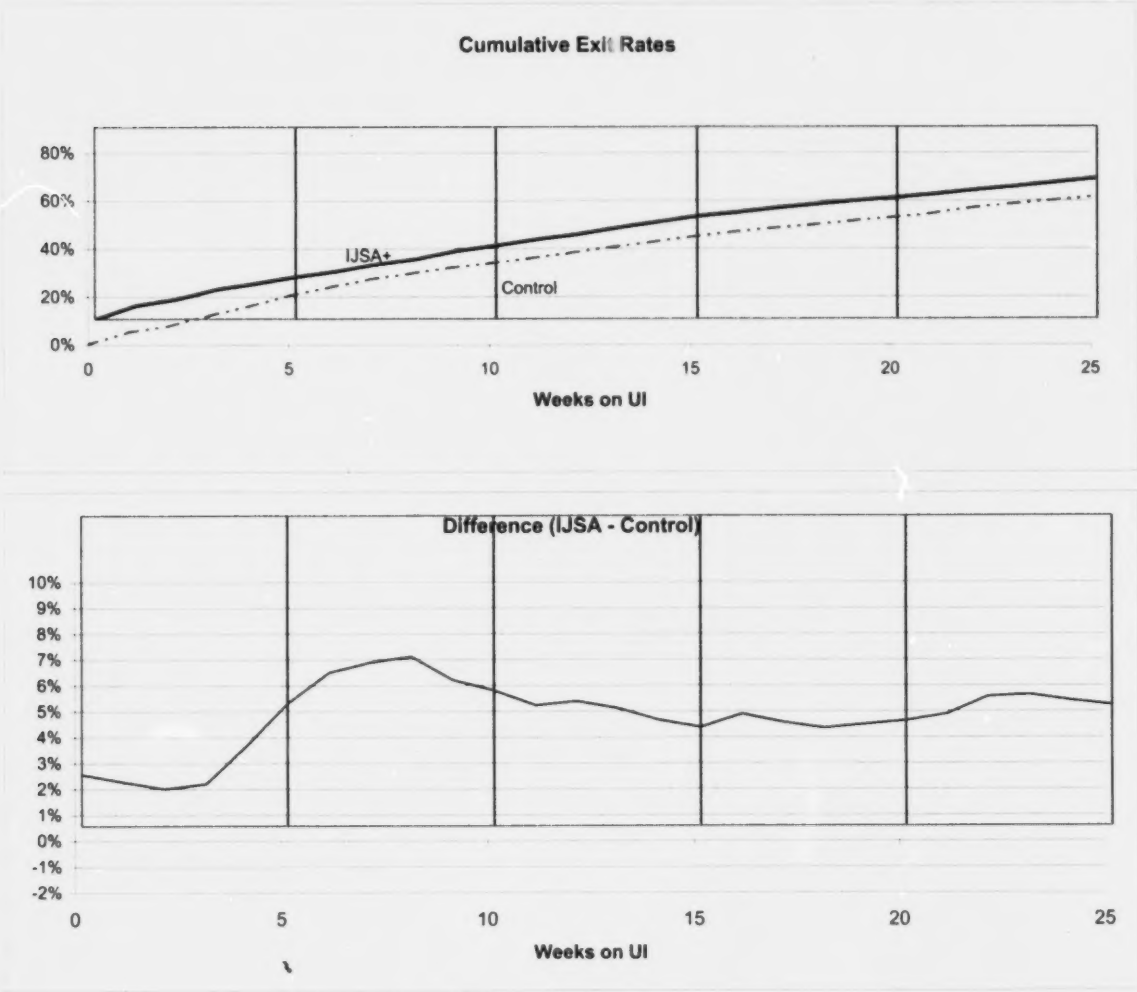

TABLE V.6

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE ON CUMULATIVE UI EXIT RATES

Length of UI Spell, in Weeks	Control Group Exit Rate (Percent)	IJSA		IJSA +	
		Treatment Group Exit Rate (Percent)	Estimated Impact	Treatment Group Exit Rate (Percent)	Estimated Impact
		DISTRICT OF COLUMBIA			
5	11.6	14.8	3.2***	15.8	4.2***
10	20.1	22.8	2.7**	23.1	3.0***
15	27.9	29.3	1.4	30.6	2.7**
20	32.4	34.2	1.8	35.1	2.7**
25	39.9	41.9	2.0*	42.7	2.8**
		FLORIDA			
5	17.5	20.2	2.7***	20.2	2.8***
10	30.4	33.9	3.5***	33.7	3.2***
15	42.7	44.3	1.6	44.6	1.8*
20	50.5	52.6	2.1*	52.6	2.1*
25	58.5	61.4	2.9**	61.2	2.7**

SOURCE: State UI claims records.

NOTE: The impact estimates are based on the differences in exit rates between the control group and the IJSA and IJSA + groups.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

percentage points, compared with 6.2 percentage points for SJSA. Among the estimated impacts at five-week intervals shown in Table V.6, the impacts in weeks 5, 10, and 25 are significant in both states and across both treatments.

Although the impacts are modest, they are relatively persistent. They decay only partially and slowly after the maximum is achieved. Even out as far as week 25, the four treatment impacts across the two states are statistically significant at least at the 90 percent confidence level. The relative persistence of the impacts is especially apparent in Florida, where the IJSA and IJSA+ impacts decayed much less than the SJSA impact. The IJSA and IJSA+ treatments in Florida increased the cumulative exit rate as of week 25 by an estimated 2.9 and 2.7 percentage points, compared with a 1.0 percentage point increase for SJSA. In Florida, the lack of decay in the impacts of the individualized treatments on UI exit rates probably accounts for the large impacts of these treatments on UI weeks relative to SJSA.

C. IMPACTS ON UI RECEIPT BY CLAIMANT SUBGROUP

Although we have presented estimates of the average impacts on all claimants targeted for JSA treatments, these treatments probably had different impacts on different types of claimants. Eligible claimants differ in basic demographic characteristics--such as gender, age, race, education--in pre-UI employment experience, and in the predicted probability of exhausting their UI benefits. If the impacts of the treatments varied along these dimensions, and we can use these dimensions to define subgroups of claimants, then we can measure how the impacts on UI receipt varied across subgroups. We performed this analysis only for the initial benefit year because our estimates of the average impacts in the following calendar year suggest that there are probably no long-run impacts. We also focused on one particular UI outcome, dollars of UI payments, but the results are similar for the other outcomes.

We measured impacts separately for each subgroup for two reasons. First, it would be useful to know how the average impacts in each state would have been different had the eligibility criterion been

stricter, that is, had the minimum predicted exhaustion probability been set higher. This analysis could prove useful if the states consider reducing the number of claimants required to participate in employment services by raising the minimum exhaustion probability necessary for eligibility.[7] This subgroup analysis involves estimating *separately* the treatments impacts for three intervals of the predicted exhaustion probability. The results of this analysis reveal whether the JSA impacts varied according to the probability of exhaustion.

The second reason for measuring the impacts on subgroups separately is that while it may be difficult for the states to target mandatory services on any basis other than the predicted exhaustion probability, it may be useful to know the extent to which the treatment impacts varied according to basic demographic characteristics and pre-UI employment experiences. Suppose there was evidence that none of the treatments was effective in reducing the total amount of UI benefits paid to claimants with a college education. If states wanted to restrict services to those likely to benefit from them, then they might exclude college graduates from the requirement to participate in employment services.

Even if states are prohibited from excluding certain subgroups from the eligible population (or choose not to do so), estimates of subgroup impacts may be useful in predicting the impacts of similar programs on different populations in other states. If, for example, we learn that the treatments in D.C. and Florida are especially effective in reducing UI benefits among those previously employed in service sector jobs, then we can use the industrial mix of employment in other states to predict how effective the three treatments would be in those states.

For all variables used to define subgroups except for the probability of exhaustion,[10] the subgroup analysis controlled for all of the other variables that define subgroups for a particular set of subgroups.

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[10]Since the probability of exhaustion is a function of the other variables used to define subgroups, according to the profiling model, controlling for these other variables when computing separate impacts for different ranges on the probability of exhaustion would not make sense.

For example, when computing the impacts separately for men and women, all of the other characteristics used in the subgroup analysis were held constant to ensure that impacts related to differences in gender cannot be attributed to other variables that are both related to gender and included in the subgroup analysis (such as occupation).

Our analysis succeeded in identifying many subgroups with significant treatment impacts. In many instances, the impacts also appear to have differed greatly across subgroups. However, the samples were often too small to provide compelling statistical evidence that the apparently large and apparently small impacts were significantly different from the average impact. Furthermore, the variations in impacts by subgroup show very different patterns in D.C. and Florida. Given these findings, it is difficult to draw any strong conclusions from the subgroup analysis. In the remainder of this section, we describe the findings from this analysis, highlighting the cases in which the differences between subgroups are statistically significant. Since the patterns of subgroup estimates differ so greatly between the two states, we discuss the findings for each state separately.

1. District of Columbia

Overall, the results for D.C., presented in Table V.7, suggest that the impacts of the treatments were larger for whites and younger claimants, and smaller for claimants previously employed in technical and managerial occupations. With respect to variation in impacts by race, the strongest evidence for differences appears in the impacts of IJSA and IJSA+ on total UI benefits. The impacts

TABLE V.7

SUBGROUP IMPACTS IN THE INITIAL BENEFIT YEAR,
DISTRICT OF COLUMBIA: TOTAL BENEFITS

Subgroups	Control Mean	Impacts SJSA	Impacts IJSA	Impacts IJSA+	Sample Size
Average Impacts[a]		-185*	-52	-56	8,071
Estimated Probability of Exhaustion[b]					
1st Quartile (lowest)	4,294	-224	-110	-30	2,017
2nd Quartile	4,079	-139	-115	-65	2,018
3rd Quartile	4,368	-42	103	-25	2,018
4th Quartile (highest)	4,202	-335*	-108	-18	2,018
Gender					
Male	4,251	-159	-64	-53	3,673
Female	4,223	-207*	-41	-58	4,398
Age					
Under 35	4,225	-348*	-259*#	-187*	3,398
35 to 44	4,350	-164	99	59	2,448
45 and older	4,126	41	99	18	2,225
Race/Ethnicity					
White	4,187	-52	-591*#	-537*#	744
Black	4,317	-229*	-51	7	5,916
Hispanic	3,971	-454	97	-221	376
Other	3,900	69	277	-8	1,035
Education					
No High School Diploma	3,974	-325	-119	-18	1,140
High School Diploma Only	4,238	-154	-95	-63	4,127
College Degree	4,402	-54	12	-157	2,238
Missing	4,095	-640*	149	315	566
Industry at Previous Job					
Wholesale and Retail Trade	4,144	-367*	-75	-124	1,165
Finance, Insurance, and Real Estate	4,723	-486*	-306	-364	655
Services	4,098	-131	-63	-62	4,221
Public Administration	4,445	-180	283	-60	653
Other	4,404	-58	-35	169	1,377

Table V.7 *(Continued)*

Subgroups	Control Mean	Impacts SJSA	IJSA	IJSA+	Sample Size
Occupation at Previous Job					
Technical and Managerial	4,403	273*#	-154	136	1,664
Clerical and Sales	4,111	-236*	32	-47	4,769
Other	4,427	-502	-192	-276	1,638
Tenure					
Less than 1 Year	4,274	-176	-171	-37	2,673
1 to 3 Years	4,241	-265*	-34	-89	2,322
3 to 10 Years	4,123	-107	81	2	2,137
10 Years or More	4,371	-192	-55	-162	939
Base Year Wages					
1st Quartile (lowest)	2,410	-33	-18	-108	2,020
2nd Quartile	3,609	-223	-186	-141	2,017
3rd Quartile	4,790	-335*	-189	82	2,017
4th Quartile (highest)	6,134	-150	187	-56	2,017
Sample Size	2,012	2,026	2,022	2,011	

SOURCE: State UI claims records.

[a]These impacts were evaluated at the mean over all eligibles for each of the subgroup variables listed in the table except for the probability of exhaustion (see footnote b).

[b]In computing the impacts for different categories of this variable, we did not control for the other subgroup variables listed in this table, many of which contribute to predicting exhaustion.

*Statistically different from zero at the 95 percent confidence level in a one-tailed test.

#Statistically different from the average impact at the 95 percent confidence level in a two-tailed test.

of IJSA and IJSA+ for whites are -$591 and -$537, respectively, and both impacts are significantly different from the average effects of these treatments on benefits for all claimants (-$52 and -$56, respectively).

With respect to age, impacts of the three treatments on UI receipt tended to be largest for the youngest group--claimants under 35. For example, SJSA reduced average UI receipt among young claimants by $348, which is statistically significant at the 95 percent confidence level. Similarly, the IJSA and IJSA+ impacts on this groups were -$259 and -$187, although the latter impact is not statistically significant. In contrast, five of the six estimated impacts for the older subgroups are positive, implying that the treatments did not reduce average UI receipt among these five groups, and the one negative estimate is not statistically significant. Younger workers may have been most responsive to the treatments because of weaker ties to a particular industry or occupation.

There is no strong evidence that the treatment impacts varied according to a claimant's base year wages, industry at previous job, or tenure at previous job, but there is evidence that the impacts of SJSA varied according to the claimant's occupation at previous job. UI receipt among claimants previously employed in technical or managerial occupations appears to have been unaffected, or even raised, by the SJSA program, in contrast to claimants in other occupations. Claimants from technical and managerial occupations saw an estimated *increase* in average UI receipt of $273 (relative to the control group), which is insignificantly different from zero but significantly different from the average treatment effects of -$185. This finding may indicate that experienced technical and managerial workers are in such high demand that providing them with job search assistance is unnecessary.

Contrary to our expectations, the treatment impacts do not appear to vary much according to the predicted probability of benefit exhaustion. Prior to the demonstration, we expected that impacts would probably be largest for claimants with the highest probabilities of benefit exhaustion. For SJSA, the impact is largest for claimants in the highest probability quartile (-$335), but while the impact is significantly different than zero, it is not significantly different from the average impact for all claimants

(-$185). Furthermore, the next largest UI reduction occurred for claimants in the lowest quartile. For IJSA and IJSA+, there does not appear to be a clear relationship between the magnitude of the impacts and the probabilities of benefit exhaustion. These findings imply that making the claimant targeting more exclusive in D.C. would probably not have substantially influenced the average treatment impacts.

However, when we examine the top *decile* of claimants with respect to their exhaustion probabilities (instead of the top quartile), we find particularly large estimated impacts of SJSA. The estimated impact of SJSA on the top decile and on the bottom 90 percent are -$626 and -$134, respectively. While the difference between these two estimates is significant only at the 90 percent level, the difference between these two groups in the estimated impacts on *weeks* of UI benefits (-2.93 weeks and -0.92 weeks) is significant at the 95 percent level. These results suggest that the impacts of JSA treatments may be larger than average for claimants with very high benefit exhaustion probabilities.

2. Florida

The results for Florida overall suggest that the impacts of IJSA and IJSA+ may have been larger for women than for men, that the impacts of SJSA may have been larger for claimants previously employed in trade industries, and that the impacts of IJSA and IJSA+ for claimants previously employed in technical, managerial, clerical or sales occupations were particularly small (or zero).

Unlike DC, there is no evidence in Florida that impacts varied by age group and racial group, but there is some evidence that IJSA and IJSA+ impacts varied according to gender. Table V.8 shows that IJSA+ reduced UI payments to women by an estimated $141 but had no impact on UI receipt among men. The difference by gender is also statistically significant. The impact of IJSA varied similarly according to gender, although the magnitude of the difference is somewhat smaller than for IJSA+, and the impact for females is not statistically significant.

Although there is little evidence that the treatment impacts varied with a claimant's previous job tenure, there is some evidence that the impacts varied according to the claimant's previous industry and occupation. With respect to industry, SJSA, and perhaps IJSA, had larger impacts on claimants previously employed in trade industries than on claimants employed in other industries. For example, SJSA reduced UI receipt among claimants from trade industries by an estimated $240, compared with an average reduction of -$32 among all claimants.

Treatment impacts also varied by occupation. The UI outcomes of claimants previously employed in technical, managerial, clerical, or sales occupations seem to have been virtually unaffected by the IJSA and IJSA+ treatments, unlike outcomes for claimants previously employed in other occupations, whose UI receipt dropped substantially. For example, the impacts of IJSA and IJSA+ on UI receipt among claimants in occupations other than technical, managerial, clerical, or sales were -$181 and -$220, respectively, and the impact of IJSA+ on these claimants is also significantly different from the average IJSA+ impact on all claimants. Previous occupation is the one dimension on which the subgroup impacts in Florida are similar to those in D.C. As in D.C., experienced technical and managerial workers in Florida may have had so many reemployment opportunities that job search assistance was unnecessary. In Florida, this same logic may also apply to clerical and service workers.

TABLE V.8

SUBGROUP IMPACTS IN THE INITIAL BENEFIT YEAR,
FLORIDA: TOTAL BENEFITS

Subgroups	Control Mean	Impacts SJSA	Impacts IJSA	Impacts IJSA+	Sample Size
Average Impacts[a]		-33	-58	-68	12,042
Estimated Probability of Exhaustion[b]					
1st Quartile (lowest)	2,337	-57	-42	-10	3,005
2nd Quartile	2,379	-15	-242*	-92	3,016
3rd Quartile	2,744	64	-23	49	3,009
4th Quartile (highest)	3,439	-114	-108	-227*	3,012
Gender					
Male	2,665	-56	-9	-6	6,504
Female	2,777	-4	-115*	-141*	5,538
Age					
Under 35	2,466	47	3	-102	3,425
35 to 44	2,698	-90	-62	-6	3,424
45 and older	2,893	-47	-95	-86	5,193
Race/Ethnicity					
White	2,649	-29	-72	-77	7,401
Black	2,731	28	-14	-33	1,880
Hispanic	2,906	-102	-46	-96	2,614
Other	2,538	294	-129	428	147
Education					
No High School Diploma	2,682	-47	-95	-16	3,122
High School Diploma Only	2,724	-94	-18	-71	6,692
College Degree	2,741	173*#	-125	-131	2,228
Industry at Previous Job					
Wholesale and Retail Trade	2,688	-240*#	-203*	-70	2,012
Finance, Insurance, and Real Estate	2,754	-38	-215*	-206	1,281
Services	2,519	95	-7	-62	3,200
Manufacturing	2,703	33	-18	86	1,551
Other	2,882	-54	9	-88	3,998

Table V.8 *(Continued)*

Subgroups	Control Mean	Impacts			Sample Size
		SJSA	IJSA	IJSA+	
Occupation at Previous Job					
Technical and Managerial	2,748	137	80	-15	2,460
Clerical and Sales	2,697	-113*	-25	27	5,365
Other	2,722	-29	-181*	-220*#	4,217
Tenure					
Less than 1 Year	2,507	-16	-67	9	4,372
1 to 3 Years	2,692	-57	-114	22	3,005
3 to 10 Years	2,846	-51	0	-149*	3,464
10 Years or More	3,158	23	-51	-335*#	1,201
Base Year Wages					
1st Quartile (lowest)	1,450	-69	-64	-141	3,019
2nd Quartile	2,145	-49	52	-57	3,008
3rd Quartile	3,341	-126	-309*#	-196*	3,008
4th Quartile (highest)	3,928	115	89	123#	3,007
Sample Size	3,014	3,032	3,007	2,989	

SOURCE: State UI claims records.

[a]These impacts were evaluated at the mean over all eligibles for each of the subgroup variables listed in the table except for the probability of exhaustion (see footnote 2).

[b]In computing the impacts for different categories of this variable, we did not control for the other subgroup variables listed in this table, many of which contribute to predicting exhaustion.

*Statistically different from zero at the 95 percent confidence level in a one-tailed test.

#Statistically different from the average impact at the 95 percent confidence level in a two-tailed test.

There also appears to be some relationship between the size of the treatment impacts and base year wages in Florida. The impacts of all three treatments seem to be particularly large for claimants with base year wages in the second highest quartile, yet particularly *small* for claimants in the highest quartile. The impact of IJSA on claimants in the second highest quartile is significantly different from zero and from the average impact on all claimants, while the impact of IJSA+ is significantly different from zero but not from the average impact.

As in D.C., we found no strong evidence in Florida that the treatment impacts were consistently larger for claimants with higher predicted probabilities of benefit exhaustion. For two of the three treatments, SJSA and IJSA+, the largest estimated impact occurred for the highest probability quartile. However, the SJSA impact is not significantly different from zero, and the IJSA+ impact, while significantly different from zero, is not significantly different from the average impact across all claimants. Unlike the situation in D.C., we found no evidence in Florida that the impacts were particularly large for claimants in the top decile of the predicted benefit exhaustion probabilities. These findings, together with the findings from D.C., demonstrate that targeting the JSA treatments to claimants with the highest predicted benefit exhaustion probabilities would not have substantially affected the average treatment impacts.

VI. IMPACTS ON UI BENEFIT DETERMINATIONS AND DENIALS

Claimants assigned to one of the JSA treatment groups were required to participate in JSA services. Claimants who refused to participate in services were at risk of being denied at least part of their benefits. Enforcement of the JSA participation requirements was designed to occur through the procedures used to enforce normal UI eligibility requirements. To track JSA participation among claimants assigned to the demonstration, each state added an eligibility requirement based directly on demonstration participation and tracked it throughout the demonstration. Because the JSA treatments established requirements to participate in various services, we expected that the treatments would increase the frequency with which the states evaluated claimants' eligibility for benefits. We also expected that the demonstration requirements would lead to more frequent benefit denials.

In this chapter, we present our estimates of the impacts of the JSA treatments on UI benefit eligibility determinations and denials. We estimated these impacts by comparing the frequency of determinations and denials for the control group with that of the JSA treatment groups. Our findings demonstrate that all of the treatments increased the frequency of determinations and denials. Many of these impacts are not directly attributable to the enforcement of the demonstration requirements. Our findings show that the enforcement of the demonstration requirements increased the frequency of determinations and denials not directly related to the demonstration as well as those due to demonstration nonparticipation. It appears that the demonstration states used additional information gathered through the demonstration to increase enforcement of traditional UI eligibility requirements.

A. BENEFIT DETERMINATIONS AND DENIALS AMONG THE CONTROL GROUPS

The control groups in D.C. and Florida had generally similar experiences with respect to the likelihood of benefit determinations and denials. The frequencies of determinations and denials for the control group and for the SJSA group are shown in Table VI.1. In each state, about 25 to 30 percent of control group claimants had at least one nonseparation, nonmonetary benefit determination, with the probability of a determination being slightly higher in D.C. About 9 or 10 percent of control group claimants had at least one denial.

Although the likelihood of having at least one determination was broadly similar in the two states, the average number of determinations per claimant was considerably higher in D.C. than in Florida--0.61 determinations per claimant in the D.C. control group compared with 0.41 in the Florida control group. The combination of these findings suggests that D.C. claimants were more likely than claimants in Florida to have multiple determinations. Indeed, as shown in Table VI.1, 12.8 percent of D.C. control group claimants had more than one determination, compared with 8.1 percent of Florida control group claimants.

Although the average number of determinations per claimant for the control groups differed from state to state, the number of denials per claimant was similar for the control groups in the two states--0.12 denials per claimant in D.C. and 0.13 denials per claimant in Florida. Combining the determination and denial findings demonstrates that the probability of a determination resulting in a denial was higher in Florida than in D.C. As shown in Table VI.1, 31 percent of determinations resulted in denials for the Florida control group, while only 20 percent resulted in denials in D.C.

Our findings suggest that D.C. tended to be a bit more aggressive than Florida in conducting benefit determinations for the type of claimants targeted by the demonstration. At the same time,

TABLE VI.1

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE ON NONSEPARATION, NONMONETARY DETERMINATIONS AND DENIALS

Outcome	District of Columbia			Florida		
	Control Group	SJSA Group	Estimated Impact[a]	Control Group	SJSA Group	Estimated Impact[a]
Determinations						
Percent with at Least One	30.2	66.8	36.6*** (1.5)	26.9	31.2	4.4*** (1.2)
Percent with More Than One	12.8	36.1	23.3*** (1.3)	8.1	11.1	3.1*** (0.8)
Average Number per Claimant	0.61	1.39	0.78*** (0.05)	0.41	0.53	0.12*** (0.03)
Denials						
Percent with at Least One	10.3	21.1	10.8*** (1.1)	8.7	11.6	2.9*** (0.8)
Percent with More Than One	1.7	3.1	1.4*** (0.5)	2.2	3.0	0.8** (0.4)
Average Number per Claimant	0.12	0.25	0.13*** (0.01)	0.13	0.17	0.05*** (0.01)
Percent of Determinations Leading to Denial	19.9	17.8		31.3	33.0	
Sample Size	2,012	2,026		3,014	3,032	

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the SJSA group.

[a]Standard errors in parentheses.

**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

the states denied benefits at about the same rate, so more aggressive determinations in D.C. did not translate into more frequent benefit denials.

B. IMPACTS ON DETERMINATIONS AND DENIALS

1. Structured JSA

As expected, the probability of benefit determination in both D.C. and Florida was higher for the SJSA group than for the control group, but the differences varied greatly between the two states. The difference in determinations was much larger in D.C., where the probability of determination was 36.6 percentage points higher for the SJSA group than for the control group (Table VI.1), and the difference is statistically significant at the 99 percent confidence level. The corresponding difference in Florida was considerably smaller at 4.4 percentage points, although it is still statistically significant.

The probability of benefit denial was also significantly higher for the SJSA groups than for the control groups. Again, the larger impact occurred in D.C. The probability of having at least one denial in D.C. was 10.8 percentage points higher for the SJSA group than for the control group. The corresponding difference in Florida was smaller at 2.9 percentage points.

2. Individualized JSA

Our findings for the impact of IJSA and IJSA+ on determinations and denials generally parallel our findings for SJSA. The impacts were larger in D.C., where both IJSA and IJSA+ increased the proportion of claimants having at least one benefit determination by about 29 percentage points, as shown in Table VI.2. These impacts represent a doubling of the determination rate for the control group, and they are both statistically significant at the 99 percent confidence level. IJSA and IJSA+

TABLE VI.2

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE TREATMENTS ON NONSEPARATION, NONMONETARY DETERMINATIONS AND DENIALS, DISTRICT OF COLUMBIA

		IJSA		IJSA +	
Outcome Measure	Control Group Outcome	Treatment Group Outcome	Estimated Impact	Treatment Group Outcome	Estimated Impact
Determinations					
Percent with at Least One	30.2	59.2	29.0*** (1.5)	58.9	28.7*** (1.5)
Percent with More Than One	12.8	26.0	13.1*** (1.2)	27.6	14.8*** (1.2)
Average Number per Claimant	0.61	1.06	0.45*** (0.04)	1.11	0.51*** (0.04)
Denials					
Percent with at Least One	10.3	18.4	8.1*** (1.1)	17.3	7.0*** (1.1)
Percent with More Than One	1.7	2.5	0.8** (0.5)	3.1	1.4*** (0.5)
Average Number per Claimant	0.12	0.21	0.09*** (0.01)	0.21	0.09*** (0.01)
Percent of Determinations Leading to Denial	19.9	20.1		18.7	
Sample Size	2,012	2,022		2,011	

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the IJSA and IJSA + groups.

**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

also significantly increased the proportion of D.C. claimants with multiple determinations, again by doubling the control group rate.

In Florida, IJSA and IJSA+ increased the probability of a benefit determination by about 3 percentage points, as shown in Table VI.3. Although these impacts are much smaller than in D.C., they are still statistically significant at the 95 percent confidence level. Both treatments also significantly increased the proportion of claimants with multiple determinations and the average number of determinations per claimant.

In both states, IJSA and IJSA+ also increased the rate of benefit denial. In D.C., the estimated increases, which are presented in Table VI.2, were 8 percentage points and 7 percentage points for IJSA and IJSA+, respectively. The estimated increases in Florida--3 percentage points for IJSA and 2 percentage points for IJSA+--were smaller but still statistically significant.

The impacts of IJSA and IJSA+ on determinations and denials tended to be smaller than the impacts of SJSA. For example, although the impacts of IJSA and IJSA+ on determinations (both equal to about 29 percentage points) were large in D.C., the impact of SJSA (37 percentage points) was still substantially larger. Similarly, the impact of SJSA on denials in D.C. was greater than the impact of IJSA or IJSA+. In Florida, the SJSA impacts were larger than the IJSA or IJSA+ impacts, but all of the impacts tended to be small, and the differences between the treatments in Florida were more modest than in D.C.

The finding that IJSA and IJSA+ tended to generate smaller increases in determinations and denials than did SJSA is consistent with our expectations. SJSA required the average claimant to participate in more services and make more contacts with the demonstration than did the IJSA and IJSA+ treatments. Therefore, claimants in the SJSA group had a greater risk of jeopardizing their benefit eligibility by not complying with the treatment requirements.

TABLE VI.3

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE TREATMENTS ON NONSEPARATION, NONMONETARY DETERMINATIONS AND DENIALS, FLORIDA

		IJSA		IJSA +	
Outcome Measure	Control Group Outcome	Treatment Group Outcome	Estimated Impact	Treatment Group Outcome	Estimated Impact
Determinations					
Percent with at Least One	26.9	29.5	2.7** (1.2)	29.6	2.8*** (1.2)
Percent with More Than One	8.1	10.1	2.0*** (0.7)	10.5	2.4*** (0.7)
Average Number per Claimant	0.41	0.47	0.06*** (0.02)	0.49	0.09*** (0.02)
Denials					
Percent With at Least One	8.7	11.7	3.0*** (0.8)	10.8	2.0*** (0.8)
Percent With more Than One	2.2	2.9	0.7** (0.4)	2.6	0.5 (0.4)
Average Number per Claimant	0.13	0.17	0.04*** (0.01)	0.16	0.03** (0.01)
Percent of Determinations Leading to Denial	31.3	36.1		32.2	
Sample Size	3,014	3,007		2,989	

SOURCE: State UI claims records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the IJSA and IJSA + groups.

**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

C. DETERMINATIONS AND DENIALS BY REASON

In this section, we investigate the UI benefit determinations and denials by reason. Our investigation is intended to shed light on the way determinations and denials were used to enforce the demonstration participation requirements.

1. Structured JSA

We began our investigation of determinations and denials by examining the determinations by reason for the control group and SJSA group, which are shown in Table VI.4. Since both states had a separate code for demonstration-related determinations and denials, we expected that the treatment-control differences would be fully explained by this code. Indeed, in D.C., much of the large overall difference in determinations between the control group and the SJSA group was attributable to the 1,453 determinations related to JSA participation issues. These determinations represent more than half of all determinations conducted for the SJSA group, and they represent 91 percent of the overall difference between the groups in the number of determinations.

But Table VI.4 also reveals one more notable difference between the D.C. groups. The number of determinations that occurred because claimants might have been employed ("not unemployed") and therefore not eligible for UI was more than four times higher for the SJSA group than for the control group. This difference occurred even though "not unemployed" as a reason for the determination was not directly linked to the requirements of the demonstration.

In Florida, there were many fewer determinations that were directly attributable to JSA than in D.C. Only 177 JSA-related benefit determinations were conducted in Florida. Furthermore, this difference accounts for only about half of the overall difference between the groups, which was equal to about 400 extra determinations for the SJSA group. The remaining difference is primarily a

TABLE VI.4

NONSEPARATION, NONMONETARY DETERMINATIONS AND DENIALS
BY REASON, STRUCTURED JOB SEARCH ASSISTANCE GROUPS

Reason	District of Columbia				Florida			
	Determinations		Denials		Determinations		Denials	
	Control Group	SJSA Group	Control Group	SJSA Group	Control Group	SJSA Group	Control Group	SJSA Group
JSA Participation	NA	1,453	NA	61	NA	177	NA	53
Able and Available	215	179	39	35	425	686	168	265
Other Reporting	49	65	31	31	60	54	39	32
Not Unemployed	76	333	63	283	200	179	83	75
Other	885	790	111	92	543	508	94	104
Total	1,225	2,820	244	502	1,228	1,604	384	529

SOURCE: State UI claims records.

NA = Not Applicable

result of the fact that the frequency of determinations made because a claimant was "able and available" was higher for the SJSA group than for the control group.[1]

Findings from both states on determinations by reason suggest that in both states, the local program operators used the extra information gathered as part of JSA enforcement to enforce other UI eligibility requirements. The demonstration therefore introduced two elements that affected determinations. First, it introduced new requirements that claimants needed to fulfill to avoid having their benefits denied. Second, it provided additional information that was used to check on other dimensions of UI eligibility.

The effect of SJSA on determinations in D.C. also had important implications for benefit denials. As described above, denials were significantly more likely for the SJSA groups than for the control groups. Table VI.4 shows that the number of denials was twice as high for the SJSA group as for the control group. But only about a quarter of the difference is attributable directly to the JSA requirements. Most of the extra denials occurred because a claimant was "not unemployed." The number of denials for this reason was four times higher for the SJSA group than for the control group. It therefore appears that in D.C. an important effect of the demonstration was to provide extra information on claimants who were found to be working and therefore ineligible for benefits.

In Florida also, the difference in denials between the SJSA group and the control group is not primarily explained by benefits denied due to lack of JSA participation. The SJSA group had 529 benefit denials compared with 384 for the control group. About one-third of the extra denials were tied to the JSA requirements, and about two-thirds were due to stricter enforcement of the "able and available" requirement for the SJSA group as compared with the control group. As in D.C., the

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[1] "Able and available" issues relate to the requirement that a UI claimant be able and available to work in any week for which they claim benefits.

stricter enforcement of these eligibility requirements was probably made possible because of the additional information provided by the demonstration.

2. Individualized JSA

Again, the findings for individualized JSA parallel those for SJSA, but the differences are smaller in magnitude. Table VI.5 shows that many D.C. claimants assigned to the individualized treatments--848 in the IJSA group and 921 in the IJSA+ group--had benefit determinations that were directly related to the JSA participation requirements. But these figures are substantially lower than the 1,453 determinations for the SJSA group (Table VI.4). That the IJSA and IJSA+ groups had fewer determinations is not surprising given that the SJSA treatments had more requirements for most claimants. We therefore expected more of the SJSA claimants to face benefit determinations due to nonparticipation in services.

IJSA and IJSA+ claimants in D.C. were also more likely than the control group to have benefit determinations because they were deemed "not unemployed". What was appears to be true for SJSA also appears to be true for IJSA and IJSA+: that enforcement of the participation requirements complemented the enforcement of the traditional UI eligibility requirements.

Some claimants in the Florida IJSA and IJSA+ groups had benefit determinations directly tied to the demonstration, although the number of those who did is substantially lower than in D.C. In Florida there were 84 determinations for the IJSA group and 102 for the IJSA+ group, which represents less than one-eighth of the corresponding demonstration-related determinations in D.C. shown in the same table. Not all of the treatment-control differences in determinations are due to the demonstration-specific determinations. The IJSA and IJSA+ groups had 278 and 230 "able and available" determinations, compared with the control group, which had only 168 such determinations.

TABLE VI.5

NONSEPARATION AND NONMONETARY DETERMINATIONS BY REASON, INDIVIDUALIZED JOB SEARCH ASSISTANCE GROUPS

Reason	District of Columbia			Florida		
	Control Group	IJSA Group	IJSA+ Group	Control Group	IJSA Group	IJSA+ Group
JSA Participation	NA	848	921	NA	84	102
Able and Available	215	154	159	425	621	605
Other Reporting	49	72	77	60	51	53
Not Unemployed	76	263	251	200	183	175
Other	885	798	833	543	481	538
Total	1,225	2,135	2,241	1,228	1,420	1,473

SOURCE: State UI claims records.

NA = Not Applicable

The benefit denials for the IJSA and IJSA+ groups are organized by reason in Table VI.6. In both states, the differences in benefit denials between the control group and the IJSA and IJSA+ groups were primarily attributable to issues not directly related to the demonstration. In D.C., most of the extra denials were due to stricter enforcement of the regular UI "not unemployed" requirement. In Florida, most of the extra denials were due to enforcement of the regular UI "able and available" requirement. These findings closely parallel our findings for the impacts of SJSA in both states, which also increased denials in D.C. due to the "not unemployed" requirement and denials in Florida due to the "able and available" requirement.

More benefit denials, whether or not directly related to the demonstration, account for some part of the reduction in UI receipt caused by the treatments. The increase in benefit denials for D.C. claimants who were employed is especially significant because this type of denial disqualifies the claimant for the entire claim rather than for a week of benefits, which is the case for most UI eligibility issues and was the case for JSA issues. Under a given set of assumptions, we calculated the UI savings generated by these denials to be 0.88 weeks per claimant in the SJSA group, which represents about 78 percent of the observed overall reduction of 1.13 weeks attributed reduction of 1.13 weeks attributed to SJSA.[2] Admittedly, this is a crude estimate and

[2]In D.C., 263 of 2,026 SJSA claimants, or 13.0 percent, were denied benefits because they were employed compared with only 62 of 2,012 control claimants, or 3.1 percent (these numbers do not match those in Table VI.8 because some claimants had multiple denials). So the net impact of SJSA was to increase the probability of this type of benefit denial by 9.9 percentage points. In our calculations, we assumed that the 201 additional SJSA claimants who were denied benefits because of demonstration requirements received 11.2 weeks of benefits, which is the average for all 263 SJSA claimants who were denied benefits because they were employed. We also assumed that in the absence of the demonstration, these claimants would not have been denied benefits and that they would have received the average UI amount for the control group, 20.1 weeks. The UI savings generated by the demonstration-induced "not unemployed" denials is therefore (20.1 - 11.2) = 8.9 weeks per denied claimant. Multiplying the increased probability of a denial by the savings per denial yields: 8.9 *.099 = 0.88 weeks. Hence, the estimated reduction in UI weeks due just to denial for being employed is 0.88 weeks per claimant.

TABLE VI.6

NONSEPARATION AND NONMONETARY DENIALS BY REASON,
INDIVIDUALIZED JOB SEARCH ASSISTANCE GROUPS

Reason	District of Columbia			Florida		
	Control Group	IJSA Group	IJSA+ Group	Control Group	IJSA Group	IJSA+ Group
JSA Participation	NA	37	51	NA	26	29
Able and Available	39	36	30	168	278	230
Other Reporting	31	39	41	39	30	32
Not Unemployed	63	236	204	83	80	74
Other	111	81	94	94	99	110
Total	244	429	420	384	513	475

SOURCE: State UI claims records.

NA = Not Applicable

probably overestimates the true share of the impact due to benefit denials--similar calculations for the IJSA and IJSA+ groups generate UI savings due to denials that are larger than the estimated treatment impacts. Regardless, these calculations suggest that the increase in benefit denials probably played a substantial role in the UI reductions in D.C.

Whether denials would play a similar role in an ongoing mandatory job search assistance program depends on how local staff enforce the participation requirements. If staff take the participation requirements seriously, we would expect that enforcement of the requirements would generate at least some benefit denials, which would contribute to the overall benefit reductions brought about by the job search assistance program. Alternatively, staff may choose not to enforce the requirements or may go to considerable lengths to avoid benefit denials, in which case we would not expect benefit denials to contribute substantially to program-induced UI reductions.

VII. IMPACTS ON EARNINGS AND EMPLOYMENT

A key goal of the JSA demonstration was to measure the impacts of different mandatory treatments on claimants' earnings and employment experiences. The JSA demonstration was designed to quicken the pace of re-employment. If the demonstration was successful, members of the treatment groups should have had higher employment rates and earnings following their initial UI claim. In addition, if JSA enhanced the job search skills of claimants, they might have been able to locate higher-paying jobs, which would also translate into higher earnings.

To test these hypotheses, we compared the earnings and employment experiences of claimants assigned to the JSA treatment groups with those of claimants assigned to the control group. Because the JSA demonstration randomly assigned claimants to treatment and control groups, we were able to use the differences between treatments and controls on various outcomes as valid estimates of program impacts. We estimated the quarterly impacts on earnings and employment using data from UI wage records. We analyzed these quarterly data starting with the first full quarter after the claim that triggered the claimant's participation in the demonstration. This approach gave us enough data to analyze employment and earnings for 10 quarters in the District of Columbia and 12 quarters in Florida.

Our results suggest positive impacts of both structured and individualized treatments in D.C., and no impacts of these treatments in Florida. In D.C., the estimated impact of SJSA on earnings was approximately $200 per quarter, and the quarterly impact estimates are statistically significant at the 10 percent level for all quarters after the first. Following the first quarter, in which the estimated impact was small ($30) and insignificantly different from zero, the impacts were relatively stable over time. SJSA was also associated with a modest increase in the likelihood of being

employed in each quarter (about 2 to 3 percentage points), and the estimated impacts are statistically significant in about half of the quarters. Claimants in the individualized treatments (IJSA and IJSA+) had higher earnings and employment rates than controls in D.C., although the impacts were smaller than those found for SJSA. For instance, claimants in IJSA and IJSA+ in D.C. earned about $100 per quarter more than controls. The effects are statistically significant in about a third of the quarters. In Florida, JSA treatments did not have a statistically significant impact on earnings, although claimants in the IJSA and IJSA+ treatments were somewhat more likely than controls to be employed.

In the remainder of this chapter, we discuss in more detail our estimates of the impacts of the JSA treatments on employment and earnings outcomes. Included in this discussion is an analysis of how the impacts vary across different subgroups of claimants.

A. INTERSTATE DIFFERENCES IN CONTROL GROUP OUTCOMES

In this section, we describe the outcomes for members of the target population who were not required to participate in services, i.e. members of the control group. The target population in Florida fared better than the target population in D.C. Tables VII.1 and VII.3 suggest that control group claimants in Florida had higher employment rates and earnings in each quarter following their initial UI claims than did their D.C. counterparts. Table VII.3 suggests that the higher quarterly earnings in Florida are attributable to higher employment rates in that state. It is difficult to know what effect this difference might have had on the estimated impacts of the JSA demonstration in the two states. It may have been more difficult for the JSA demonstration to register impacts in Florida if job opportunities were plentiful. In that case, the enhancement of job search skills and the increase in job search intensity brought about by the JSA demonstration may have been unnecessary because claimants would have found jobs rapidly even in the absence of JSA.

TABLE VII.1

ESTIMATED IMPACTS OF STRUCTURED JSA ON QUARTERLY EARNINGS
(Dollars per Claimant)

	District of Columbia			Florida		
Quarter[a]	Control Group Mean	SJSA Group Mean	Estimated Impact	Control Group Mean	SJSA Group Mean	Estimated Impact
1	1,258	1,288	30	1,659	1,712	53
2	1,559	1,731	172**	2,203	2,199	-4
3	1,767	1,918	152**	2,592	2,539	-53
4	1,734	2,015	281***	2,673	2,671	-2
5	1,865	2,145	280***	2,881	2,789	-92
6	2,070	2,312	241**	2,955	2,890	-66
7	2,161	2,338	177*	3,034	2,978	-57
8	2,052	2,315	263**	3,071	2,974	-98
9	2,102	2,286	185*	3,165	3,067	-98
10	2,153	2,378	224**	3,224	3,201	-23
11				2,914	2,881	-33
12				2,548	2,427	-121
Total			2,005***			-592
Sample Siz	2,00 (1,759	2,02 (1,772		2,99 (2,557	3,00 (2,567	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]Full calendar quarters following initial UI claim.

[b]Sample sizes are somewhat lower for the 10th quarter for D.C. and for the 12th quarter for Florida because data for claimants who entered the demonstration during the last quarter of the demonstration were not available.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

B. IMPACTS ON EARNINGS

The JSA treatments were expected to affect earnings by quickening the pace at which claimants became re-employed and consequently increasing their total earnings. Whereas both the SJSA and IJSA treatments focused on providing job search skills, albeit in a different manner, the IJSA+ treatment was also supposed to emphasize job training when appropriate. By providing greater access to job training, the IJSA+ treatment was intended to enhance long-term earnings potential by making claimants more marketable, thereby increasing earnings. However, we found that the IJSA+ treatment did not increase the rate of participation in job training above that of claimants in the IJSA group, so we do not expect the employment and earnings impacts of these two groups to differ markedly.

In this section, we present estimates of quarterly earnings for each of the JSA treatments and controls in D.C. and Florida. We estimated the impacts by comparing the quarterly earnings and total earnings of each of the JSA treatments to those of the control group.

1. Structured JSA

In D.C., SJSA had a positive impact on earnings. As shown in Table VII.1, average earnings among SJSA claimants were higher than among the control group. Except for the first quarter, the differences are statistically significant. The impacts generally rose from $30 in the first quarter to $280 in the fourth and fifth quarters. The impacts in the sixth through tenth quarters were relatively stable, varying from $177 to $263. These results demonstrate that SJSA has a persistent effect on earnings in D.C. Overall, claimants in the SJSA treatment in D.C. earned $2,005 more than controls during the 10-quarter observation period, a statistically significant difference.

In Florida, the estimated impacts of SJSA are small and generally negative. Claimants in the SJSA earned $53 more than controls in the first quarter after filing for benefits and earned less than

controls in the remaining 11 quarters. However, none of the estimated impacts are statistically significant. Overall, claimants in SJSA earned $592 less than claimants in the control group, but this difference is not statistically significant. SJSA appears to have had little impact on earnings in Florida.

2. Individualized JSA

The impacts of IJSA and IJSA+ on earnings in D.C. were somewhat smaller and less persistent than those found for SJSA (Table VII.2). The impacts are statistically significant in quarters four through six for IJSA, and for quarters two, three, and five for IJSA+. For both treatments, the impacts peaked in the fifth quarter ($191 for IJSA and $180 for IJSA+), and then declined. Overall, claimants in IJSA earned $1,171 more than controls, a statistically significant difference. Claimants in IJSA+ earned $789 more than controls, a difference that is not statistically significant. These results indicate that individualized job search assistance had positive impacts on quarterly earnings. However, these impacts are smaller than the impacts from SJSA, and they dissipate over time.

Unlike the findings from D.C., there is no indication of any substantial impacts of IJSA or IJSA+ on earnings in Florida. Over the 12-quarter observation period, IJSA claimants earned $93 less than controls, and IJSA+ claimants earned $25 more than controls. Neither of these differences is statistically significant, nor are any of the quarterly impact estimates.

C. IMPACTS ON EMPLOYMENT RATES

In this section, we discuss the impacts of JSA on the percentage of individuals who were employed in each quarter. If JSA treatments caused individuals to obtain jobs more quickly or to remain employed at higher rates, then we would expect quarterly employment rates to be higher for the treatment groups than for the control group. In each state, we determined which demonstration

TABLE VII.2

ESTIMATED IMPACTS OF INDIVIDUALIZED JSA ON QUARTERLY EARNINGS
(Dollars per Claimant)

Quarter[a]	Control Group Mean	IJSA Group Mean	Estimated Impact	IJSA+ Group Mean	Estimated Impact
		DISTRICT OF COLUMBIA			
1	1,258	1,280	22	1,280	22
2	1,559	1,661	102	1,706	147**
3	1,767	1,877	111	1,942	176**
4	1,734	1,895	161**	1,817	83
5	1,865	2,056	191**	2,044	180**
6	2,070	2,254	183**	2,176	106
7	2,161	2,257	96	2,138	-23
8	2,052	2,180	129	2,090	38
9	2,102	2,178	76	2,112	10
10	2,153	2,254	100	2,204	50
Total			1,171*		789
Sample Size	2,006 (1,759)[b]	2,018 (1,771)[b]		2,009 (1,768)[b]	
		FLORIDA			
1	1,659	1,611	-48	1,635	-24
2	2,203	2,197	- 6	2,223	20
3	2,592	2,574	- 18	2,606	14
4	2,673	2,785	112	2,723	50
5	2,881	2,845	- 36	2,868	-12
6	2,955	2,920	-36	2,961	5
7	3,034	3,029	- 5	3,097	63
8	3,071	3,030	- 41	3,051	-20
9	3,165	3,124	- 41	3,116	-49
10	3,224	3,194	- 30	3,180	-44
11	2,914	2,920	6	2,928	14
12	2,548	2,598	50	2,557	9
Total			-93		25
Sample Size	2,997 (2,557)[b]	2,993 (2,602)[b]		2,961 (2,578)[b]	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]Full calendar quarters following initial UI claim.

[b]Sample sizes are somewhat lower for the 10th quarter for DC and for the 12th quarter for Florida because data for claimants who entered the demonstration during the last quarter of the demonstration were not available.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

participants were employed in each quarter using UI wage records. We then compared the mean employment rates of the treatment and control groups to estimate the impacts of these JSA treatments on the employment rate.

1. Structured JSA

In D.C., the estimated impacts of SJSA on quarterly employment rates are consistently positive and range between 1.3 and 3.5 percentage points, as shown in Table VII.3. The impacts are statistically significant in quarters 1, 5,6 and 8 through 10, with the largest impacts occurring in the fifth and ninth quarters. The time pattern of the results is somewhat surprising because it was anticipated that SJSA would have had its largest impacts earlier as claimants influenced by the demonstration services moved back into employment. The results in Table VII.3 indicate that the effects of SJSA on employment were larger during the second year after claimants filed for benefits. As with earnings, the estimated quarterly impacts of SJSA on employment rates are insignificant for all 12 quarters in Florida. The estimated impacts range from -1.0 percentage points in quarter 12 to 1.4 percentage points in quarter 1.

2. Individualized JSA

In general, for both DC and Florida, the estimated quarterly impacts of IJSA and IJSA+ on the employment rate are modest and statistically insignificant in most quarters. However, the impacts are largest and most consistently significant for IJSA in D.C. These impacts, shown in Table VII.4, are consistently positive and range from 1.0 percentage points in quarter 7 to 3.5 percentage points in quarter 6. The impacts in quarters 3 through 6 are larger and statistically significant. These results suggest that in D.C., IJSA took a couple of quarters to have an appreciable impact on employment, and that the impact diminished after the sixth quarter. In Florida, IJSA had a

TABLE VII.3

ESTIMATED IMPACTS OF STRUCTURED JSA ON PERCENT EMPLOYED

Quarter[a]	District of Columbia			Florida		
	Control Group Mean	SJSA Group Mean	Estimated Impact	Control Group	SJSA Group Mean	Estimated Impact
1	40.7	42.9	2.3*	49.5	50.9	1.4
2	43.1	44.6	1.5	57.7	58.6	0.8
3	45.2	46.5	1.3	62.0	62.5	0.5
4	45.8	47.7	1.9	63.8	63.2	-0.6
5	45.4	48.9	3.5**	63.4	62.8	-0.6
6	45.5	47.8	2.3*	62.8	62.5	-0.3
7	46.4	47.6	1.3	63.4	62.8	-0.7
8	45.6	48.0	2.4*	62.2	62.6	0.4
9	44.3	47.4	3.1**	61.8	62.8	1.1
10	43.7	46.5	2.8*	61.5	62.2	0.7
11				55.2	55.4	0.2
12				49.0	48.1	-1.0
Sample Size	2,006 (1,759)[b]	2,024 (1,772)[b]		2,997 (2,557)[b]	3,009 (2,567)[b]	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]Full calendar quarters following initial UI claim.

[b]Sample sizes are somewhat lower for the 10th quarter for D.C. and for the 12th quarter for Florida because data for claimants who entered the demonstration during the last quarter of the demonstration were not available.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

TABLE VII.4

ESTIMATED IMPACTS OF INDIVIDUALIZED JSA ON PERCENT EMPLOYED

Quarter[a]	Control Group Mean	IJSA Group Mean	Estimated Impact	IJSA+ Group Mean	Estimated Impact
DISTRICT OF COLUMBIA					
1	40.7	42.2	1.5	42.3	1.6
2	43.1	44.4	1.2	43.1	-0.0
3	45.2	48.1	3.0**	47.2	2.0*
4	45.8	48.5	2.7**	46.1	0.3
5	45.4	47.8	2.5*	46.3	1.0
6	45.5	48.9	3.5**	46.5	1.1
7	46.4	47.4	1.0	45.6	-0.7
8	45.6	47.2	1.6	44.8	-0.8
9	44.3	45.7	1.4	43.5	-0.8
10	43.7	44.9	1.2	43.9	0.2
Sample Size	2,006 (1,759)[b]	2,018 (1,771)[b]		2,009 (1,768)[b]	
FLORIDA					
1	49.5	50.2	0.7	49.5	0.0
2	57.7	58.4	0.7	59.9	2.2**
3	62.0	63.5	1.6	64.0	2.0*
4	63.8	65.4	1.6	63.9	0.1
5	63.4	64.0	0.6	63.1	-0.3
6	62.8	64.1	1.3	64.5	1.6*
7	63.4	64.0	0.6	64.0	0.6
8	62.2	63.8	1.6	63.6	1.4
9	61.8	63.8	2.0*	62.2	0.4
10	61.5	63.2	1.7*	62.4	0.9
11	55.2	56.4	1.2	57.2	2.0*
12	49.0	49.2	0.1	50.8	1.8
Sample Size	2,997 (2,557)[b]	2,993 (2,602)[b]		2,961 (2,578)[b]	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]Full calendar quarters following initial UI claim.

[b]Sample sizes are somewhat lower for the 10th quarter for D.C. and for the 12th quarter for Florida because data for claimants who entered the demonstration during the last quarter of the demonstration were not available.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

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somewhat smaller and later impact on employment than in D.C. Only the impacts in quarters 9 and 10 are statistically significant. However, unlike the impacts of SJSA in Florida, the impacts of IJSA are consistently positive, ranging from 0.1 percentage points to 2.0 percentage points.

In D.C., the estimated impacts of IJSA+ are smaller than those observed for IJSA. Only the impact for quarter 3 is statistically significant, and nearly half of the estimated quarterly impacts are negative. In Florida, most of the estimated impacts of IJSA+ are positive, and four of the estimated impacts are statistically significant. However, no clear pattern in the timing of these effects is evident. The largest impacts occured both in early quarters (2 and 3) and in later quarters (11 and 12).

D. IMPACTS ON EARNINGS BY CLAIMANT SUBGROUP

In sections B and C, we presented estimates of the average impacts of the JSA treatments on earnings. However, as for the impacts on UI outcomes, the impacts on labor market outcomes probably vary across different types of claimants. Therefore, we measured how these impacts vary according to basic demographic characteristics, pre-UI employment experience, and the predicted probability of exhausting UI benefits, as assigned in the profiling model. We found little compelling statistical evidence that the impacts of the treatments differ for different types of claimants. The impact estimates themselves often appear very different for different subgroups, but these differences were not measured precisely enough (due to small subgroup sample sizes) to identify many significant differences. The subgroup impacts for the initial benefit year are reported below.

1. District of Columbia

The results for the District of Columbia presented in Table VII.5 provide no compelling evidence that the impacts of the JSA treatments on earnings varied systematically across subgroups.

TABLE VII.5

SUBGROUP IMPACTS IN THE INITIAL BENEFIT YEAR, DISTRICT OF COLUMBIA: EARNINGS
(Dollars per Claimant)

Subgroups	Control Mean	Impacts			Sample Size
		SJSA	IJSA	IJSA+	
Average Impacts[a]		614*	137	197	8,071
Estimated Probability of Exhaustion[b]					
1st Quartile (lowest)	6,973	15	78	-241	2,017
2nd Quartile	7,181	914	605	244	2,019
3rd Quartile	5,426	1,110*	548	975	2,020
4th Quartile (highest)	5,750	476	296	630	2,015
Gender					
Male	5,612	809	116	-186	3,673
Female	6,315	450	156	516	4,398
Age					
Under 35	5,699	306	368	277	3,398
35 to 44	6,135	752	147	573	2,448
45 and older	6,294	931	-226	-339	2,225
Race/Ethnicity					
White	7,574	-694	709	-737	744
Black	5,817	823*	174	70	5,916
Hispanic	6,441	568	-1,351	228	376
Other	5,716	789	64	1,562*	1,035
Education					
No High School Diploma	5,647	124	318	-100	1,140
High School Diploma Only	6,101	635	192	66	4,127
College Degree	6,362	168	-91	351	2,238
Missing	4,490	3,174*#	277	1,122	566
Industry at Previous Job					
Wholesale and Retail Trade	5,587	-320	-201	880	1,165
Finance, Insurance, and Real Estate	4,216	2,121*	107	-26	655
Services	6,958	608	206	189	4,221
Public Administration	2,354	-471	-417	886	653
Other	5,963	1,217	490	-576	1,377
Occupation at Previous Job					
Technical and Managerial	5,446	1,235*	464	381	1,664
Clerical and Sales	5,667	512	-92	56	4,769
Other	7,510	281	474	419	1,638

TABLE VII.5 *(continued)*

Subgroups	Control Mean	Impacts SJSA	Impacts IJSA	Impacts IJSA+	Sample Size
Tenure					
Less Than 1 Year	6,484	719	942*	858	2,673
1 to 3 Years	6,020	547	119	-149	2,322
3 to 10 Years	5,573	149	-496	-23	2,137
10 Years or More	5.504	1,539*	-668	-328	939
Base Year Wages					
1st Quartile (lowest)	3,480	116	-637	-84	2,020
2nd Quartile	5,498	902	-115	-430	2,017
3rd Quartile	6,864	546	107	-62	2,017
4th Quartile (highest)	8,139	891	1,195*#	1,364*#	2,017
Sample Size	2,012	2,026	2,022	2,011	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]These impacts were evaluated at the mean over all eligibles for each of the subgroup variables listed in the table except for the probability of exhaustion (see footnote 2).

[b]In computing the impacts for different categories of this variable, we did not control for the other subgroup variables listed in this table, many of which contribute to predicting exhaustion.

*Statistically different from zero at the 95 percent confidence level in a one-tailed test.

#Statistically different from the average impact at the 95 percent confidence level in a two-tailed test.

The impacts do not seem to be related to the claimant's exhaustion probability for any of the three treatments. Furthermore, one of the strongest subgroup results is perhaps the most puzzling: claimants with missing education data seemed to have benefitted the most from the JSA treatments, though the difference between this subgroup and the average claimant is only significant for SJSA. The most interesting subgroup impacts appear in the base year wages of claimants. Claimants in the top quartile of base year wages seem to have received much larger-than-average earnings gains from the individualized treatments. For SJSA, the earnings impact for the top quartile is very similar to the average impact ($891 versus $614, respectively). However, the impacts of the individualized treatments on earnings are much larger for the top quartile than for the average claimant. The impacts for the top quartile are $1,195 for IJSA and $1,364 for IJSA+, compared with average impacts of $137 for IJSA and $197 for IJSA+.

In general, the subgroup analysis in D.C. provides very little evidence of interesting differences in the impacts of JSA treatments across different claimants. In fact, we found fewer subgroup impact estimates that differed significantly from the average impact than would be expected to occur by chance had the impact on earnings been the same for each claimant.

2. Florida

Like the results for D.C., the subgroup results for Florida (shown in Table VII.6) provide little compelling evidence that the impacts of JSA treatments vary systematically across claimants. For example, the impacts of the treatments do not seem to vary significantly according to the predicted probability of exhaustion. More generally, we found no more significant differences between different subgroups than would be expected to occur by chance if the impacts were the same for all claimants. Therefore, the few apparently interesting subgroup results described below should be interpreted with caution. The individualized treatments seemed to raise earnings for women but

TABLE VII.6

SUBGROUP IMPACTS IN THE INITIAL BENEFIT YEAR, FLORIDA: EARNINGS
(Dollars per Claimant)

Subgroups	Control Mean	Impacts SJSA	Impacts IJSA	Impacts IJSA+	Sample Size
Average Impacts[a]		49	194	10	12,042
Estimated Probability of Exhaustion[b]					
1st Quartile (lowest)					
2nd Quartile	8,950	755	-191	605	3,005
3rd Quartile	9,162	-395	-388	-394	3,016
4th Quartile (highest)	9,607	-256	-126	-144	3,012
	8,825	-158	804	139	3,009
Gender					
Male	10,133	20	-159	-662#	6,504
Female	7,993	84	609	799*#	5,538
Age					
Under 35	8,994	599	65	701	3,425
35 to 44	9,840	-34	465	-70	3,424
45 and older	8,796	-258	101	-391	5,193
Race/Ethnicity					
White	9,524	-171	-58	-76	7,401
Black	8,914	262	-16	-506	1,880
Hispanic	8,274	655	1,030*	726	2,614
Other	8,788	-2,360	728	-1,801	147
Education					
No High School Diploma	7,957	22	429	120	3,122
High School Diploma Only	8,907	263	-52	181	6,692
College Degree	11,543	-554	605	-656	2,228
Industry at Previous Job					
Wholesale and Retail Trade	8,589	1,133*	1,027	384	2,012
Finance, Insurance, and Real Estate	9,457	732	1,155	995	1,281
Services	9,175	-51	-416	-184	3,200
Manufacturing	10,697	-1,286*	-55	-1,492*#	1,551
Other	8,709	-115	54	247	3,998
Occupation at Previous Job					
Technical and Managerial	10,248	38	800	-54	2,460
Clerical and Sales	9,076	277	-140	-342	5,365
Other	8,601	-234	267	497	4,217

TABLE VII.6 (*continued*)

Subgroups	Control Mean	Impacts			Sample Size
		SJSA	IJSA	IJSA+	
Tenure					
Less Than 1 Year	11,295	452	-608#	-273	4,372
1 to 3 Years	8,633	614	1,041*	-330	3,005
3 to 10 Years	7,653	79	217	88	3,464
10 Years or More	6,973	366	916	1,663*#	1,201
Base Year Wages					
1st Quartile (lowest)	5,202	572	387	96	3,019
2nd Quartile	7,463	136	-8	110	3,008
3rd Quartile	9,397	194	655	564	3,008
4th Quartile (highest)	14,533	-706	-257	-730	3,007
Sample Size	3,014	3,032	3,007	2,989	

SOURCE: State UI wage records.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

[a]These impacts were evaluated at the mean over all eligibles for each of the subgroup variables listed in the table except for the probability of exhaustion (see footnote 2).

[b]In computing the impacts for different categories of this variable, we did not control for the other subgroup variables listed in this table, many of which contribute to predicting exhaustion.

*Statistically different from zero at the 95 percent confidence level in a one-tailed test.

#Statistically different from the average impact at the 95 percent confidence level in a two-tailed test.

lower earnings for men. The impacts of IJSA+ are +$799 for women and -$662 for men, and the difference is statistically significant. We found a similar, though smaller, difference in impacts by gender for IJSA.

The individualized treatments also seemed to produce larger earnings impacts for claimants with longer pre-UI job tenure than for claimants with shorter job tenure. The impact of IJSA on earnings is negative only for claimants with less than one year of pre-UI job tenure (-$608), and this estimate is significantly different from our estimate of the average impact across all job tenure categories (+$194). The impact of IJSA+ is only positive and significantly different from zero for claimants with 10 or more years of job tenure (+$1,663), and this estimate is significantly different from our estimate of the average impact (+$10).

VIII. IMPACTS ON POST-UI JOB CHARACTERISTICS

Another way to judge the impact of JSA is to look at the characteristics of jobs obtained by JSA claimants. We are specifically interested in those job characteristics that lend some insight into the quality of post-UI employment. Although a number of factors can be considered indicators of job quality, we consider wages and fringe benefits to be good indicators of job quality as well as being easy to measure. Consequently, we gauge impacts based on these outcomes.

The anticipated impact of JSA on the quality of post-UI employment is ambiguous. By encouraging rapid re-employment, JSA could cause some claimants to take "any" job and might therefore result in lower quality post-UI employment. Alternatively, by improving the job search skills of claimants, JSA could enable them to find higher quality jobs. One evaluation of a JSA-type program in New Jersey (Corson et al. 1989) considered the impact of JSA on job quality by analyzing hourly wages in post-UI jobs. In this chapter, we paint a fuller picture of the impacts of JSA on job quality by considering the impacts of JSA on claimants' health insurance and pension benefits in addition to hourly wages. We use health insurance and pension benefits to measure job quality because these are probably the two most important fringe benefits in terms of contributing to an employee's well-being.

In addition to the relationship between JSA and job quality, we also consider whether JSA increases the likelihood of claimants switching to a new occupation. Although not strictly a measure of job quality, changing occupations is an important possible outcome, especially because the type of worker targeted by the JSA demonstration may have skills that are no longer in demand and may therefore need to switch occupations. Like employment in general, the anticipated effect of JSA on the occupations of claimants is also ambiguous. On one hand, we might expect claimants who were

exposed to the JSA treatment to be more aware of opportunities in other occupations and thus more likely to switch occupations. Alternatively, the job search skills claimants acquire may enable them to find employment in their old occupation, even if there are now fewer opportunities in that occupation.

Our methods for assessing the impacts of JSA on post-UI job characteristics differ from the methods we used to obtain estimates of program impacts. For those other outcomes, the random assignment of claimants to treatment and control groups allowed us to make unbiased estimates of the impact of JSA. But here, the outcomes of interest are the characteristics of the claimant's post-UI job, and this limits our sample to those who obtained a job, potentially invalidating the treatment and control groups for estimating program impacts. To control for observable differences that may exist between the employed treatment group and control group members, we used multivariate regression techniques. To the extent that unobservable differences still exist between the treatments and the controls, our results may remain somewhat biased.

To examine the impacts of the JSA demonstration on job characteristics, we focused on the job claimants held one year after their initial UI claim. Information about this job was drawn from the follow-up survey. Survey respondents who did not hold a job one year after their initial UI claim were not included. Since the analysis is based only on survey respondents who held a job one year after their initial claim, the sample sizes are relatively small, and the estimates are imprecise. Keeping this in mind, we still note substantial impacts even when they fail to achieve conventional levels of statistical significance.

Another issue associated with using the survey data is the potential for nonresponse bias. In Appendix A, we evaluate the potential effects of survey nonresponse and find some evidence that treatment impacts on outcomes associated with employment and earnings may be somewhat

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overstated when based on the survey sample. Since some data, such as information on job characteristics, are available only from the survey, we still want to use the survey sample for some pieces of analysis. However, in interpreting the findings, we are cognizant of the fact that nonresponse bias may prompt us to overstate the impacts.

Our estimates provide little evidence that the JSA treatments encouraged clients to take lower-quality jobs than they would have without the treatments. Typically, the estimated treatment impacts on hourly wages and job benefits were positive and statistically insignificant. Some scattered impacts were positive and significant. For example, SJSA in D.C. generated an estimated increase in hourly wages of $0.99. The structured and individualized treatments each generated a significant increase in the rate at which claimants received health insurance benefits or pension benefits in at least one state. Based on these findings, we conclude that the treatments, if anything, led claimants to take higher-quality jobs rather than lower-quality jobs.

Our findings also suggest that the JSA had relatively little impact on occupational choice. When claimants had difficulty obtaining employment in the same occupation they had before they were laid off, JSA does not appear to have lessened this hardship.

Viewed together, these findings suggest that claimants in the treatment groups either started their job search earlier or searched more intensively, but that they did not lower their reservation wage or any other standard for an acceptable job offer. The positive impacts on hourly wages for SJSA in D.C. are probably at least partially responsible for the persistent earnings impacts found for SJSA, as described in the previous chapter. These findings suggest that claimants in SJSA in D.C. were induced to find relatively permanent, higher-paying jobs.

A. IMPACTS ON HOURLY WAGES

The primary indicator of job quality is the hourly wage. The regression-based estimate presented in Table VIII.1 implies that SJSA increased the post-UI hourly wages of claimants in D.C. by $0.99. This estimated impact is both large, representing nearly 10 percent of the control group mean ($10.61), and statistically significant at the 99 percent confidence level. In contrast, the employed SJSA claimants in Florida experienced an impact on the hourly wage of only $0.02, which is not statistically significant. These results are consistent with the results we presented in Chapter VII that showed significant impacts due to SJSA on quarterly earnings in D.C. but little evidence of impacts in Florida. These results also suggest that the impacts on earnings in D.C. were due to both impacts on wages (presented here) and impacts on employment (presented in Chapter VII).

For the individualized treatments, we present estimated impacts based on combining the IJSA and IJSA+ groups. This method of presentation is based explicitly on the design of the survey. Since there was little distinction between IJSA and IJSA+ in the field, the survey was designed so that the IJSA and IJSA+ groups were interviewed at half the rate of the SJSA and control groups. The intent was to combine the IJSA and IJSA+ respondents into a single group comparable in size to the SJSA or control group respondents.

Our estimates provide no clear evidence that the individualized treatments had any effect on wages. Other things being equal, hourly wages were generally higher for the combined individualized treatment groups than for the control group, by $0.07 in D.C. and by $0.57 in Florida, but neither difference is statistically significant.

TABLE VIII.1

ESTIMATED IMPACTS OF TREATMENTS ON HOURLY WAGES IN POST-UI JOB
(Standard Errors in Parentheses)

	District of Columbia	Florida
SJSA Impact	\$0.99***	\$0.02
	(0.37)	(0.38)
Combined IJSA and IJSA+ Impact	\$0.07	\$0.57
	(0.37)	(0.38)
Control Group Mean	\$10.61	\$10.19
Sample Size	901	1,260

SOURCE: JSA demonstration follow-up survey.

***Statistically significant at the 99 percent confidence level in a two-tailed test.

B. IMPACTS ON HEALTH INSURANCE BENEFITS

Recent policy debates on the financing of health care attest to the importance of this benefit. For many employees, it gauges of the quality of post-UI employment. In this section, we examine the impact of JSA on health insurance benefits.

Table VIII.2 shows that SJSA had a sizable impact on the likelihood of D.C. claimants obtaining a job with health insurance benefits. The SJSA claimants had an 11.3 percentage point greater probability of obtaining a job that provided health insurance than the control group; this impact is statistically significant at the 99 percent confidence level. In Florida, however, there is no evidence that SJSA increased the likelihood that claimants would obtain a job with health insurance benefits. On the contrary, the SJSA claimants had a 1.3 percentage point lower probability of obtaining a position with health benefits, but the difference is statistically insignificant.

The SJSA impacts on health insurance benefits parallel those for hourly wages. Although the small sample size makes our estimates less than definitive, the evidence leans toward suggesting that in D.C., SJSA claimants earned higher hourly wages and were more likely than the control group claimants to receive health benefits. In Florida, no positive impacts were found for either outcome. These results suggest that the SJSA treatment in D.C. was effective in getting claimants into better jobs, while this was not the case in Florida. This may be an indication that D.C. has more jobs with high wages and health benefits than Florida, and that these jobs are available to UI claimants who are encouraged to search intensely for re-employment.

The claimants assigned to the individualized treatments tended to find jobs with health insurance at a higher rate than did the control group, as reflected in the positive impact estimates shown in Table VIII.2. The estimates tend to be fairly large--about 10 percentage points in D.C. and 5 percentage points in Florida. However, only the D.C. estimate is statistically significant. The

TABLE VIII.2

ESTIMATED IMPACTS OF TREATMENTS ON PROBABILITY OF RECEIVING HEALTH INSURANCE ON POST-UI JOB
(Percentage Points)

	District of Columbia	Florida
SJSA Impact	11.3***	-1.3
Combined IJSA and IJSA+ Impact	9.6**	4.6
Control Group Mean (Percent)	40.1	38.0
Sample Size	945	1,316

SOURCE: JSA demonstration follow-up survey.

NOTE: Impacts shown are the marginal effects of the treatments calculated based on logit regression estimates. Statistical significance results are based on hypothesis tests of the logit coefficients.

**Statistically significant at the 95 percent confidence level in a two-tailed test.
***Statistically significant at the 99 percent confidence level in a two-tailed test.

substantial but insignificant estimate for Florida reflects the limitations created by the relatively small survey samples and the resulting imprecision of our estimates.

C. IMPACTS ON PENSION BENEFITS

Pension benefits represent a significant source of income for many retired Americans. Pension benefits can determine whether workers spend their retirement years comfortably or in poverty, or they can determine whether workers are able to retire at all. Given the importance of this benefit, it can serve as an important indicator of job quality. In this section, we examine the impact of JSA on pension benefits.

Table VIII.3 shows the SJSA treatment group in D.C. had a probability of obtaining a job with pension benefits that was 4.9 percentage points higher than the control group's probability of obtaining such a job; the corresponding figure in Florida is 4.0 percentage points. These impacts, however, are not statistically significant. Nevertheless, the magnitude of the point estimates is not trivial, as the estimates represent up to a 20 percent increase over the control group mean. Given the small sample sizes for our survey sample, our estimates are relatively imprecise, and it is not surprising that even sizable estimates therefore become statistically insignificant.

The results for pension benefits presented in Table VIII.3 are consistent with the SJSA impacts we observed in D.C. for hourly wages and health insurance benefits, where the impacts observed for both were substantial. Thus, the substantial, albeit insignificant, impacts for pension benefits in D.C. are consistent with the overall pattern. In Florida, the impact of SJSA on pension benefits runs somewhat counter to what we observed on other measures of job quality. On both hourly wages and health insurance benefits, the SJSA had a zero or even a negative, albeit insignificant, impact. In the case of pension benefits in Florida, the impact was positive, although also insignificant.

TABLE VIII.3

ESTIMATED IMPACTS OF TREATMENTS ON PROBABILITY OF RECEIVING PENSION BENEFITS ON POST-UI JOB
(Percentage Points)

	District of Columbia	Florida
SJSA Impact	4.9	4.0
Combined IJSA and IJSA+ Impact	8.2**	8.5***
Control Group Mean (Percent)	23.2	24.7
Sample Size	947	1,316

SOURCE: JSA demonstration follow-up survey.

NOTE: Impacts shown are the marginal effects of the treatments calculated based on logit regression estimates. Statistical significance results are based on hypothesis tests of the logit coefficients.

**Statistically significant at the 95 percent confidence level in a two-tailed test.
***Statistically significant at the 99 percent confidence level in a two-tailed test.

The individualized treatments appear to have increased the likelihood of receiving pension benefits on the new job. Both of the estimated impacts of the combined individualized treatments shown in Table VIII.3 are positive and statistically significant at least at the 95 percent confidence level. These estimates suggest that the treatments increased pension benefit receipt by about 8 percentage points in both states.

D. IMPACTS ON OCCUPATIONAL RETENTION

Of particular interest to policymakers is whether the services provided in the JSA demonstration led workers to switch occupations. Given that many of the claimants were dislocated workers, choosing another occupation might be a fruitful strategy for obtaining employment. On the other hand, the JSA demonstration might have enabled claimants to locate hard-to-find jobs in their old occupations. In this section, we address these questions by analyzing the impacts of the JSA demonstration on occupations.

Before estimating the impacts of JSA on occupational retention, we examine the post-UI occupational distributions of the JSA claimants by treatment and control group. The occupational distributions, which are shown in Table VIII.4, tend to be broadly similar across the groups, with the magnitude of any differences being fairly small. These statistics suggest that none of the treatments had much of an impact on occupations. In D.C., the most popular post-UI occupations across all groups were professional, clerical, and service jobs. Clerical jobs were also popular in Florida, but the claimants in other jobs, excluding laborers, were spread fairly evenly across the remaining occupations.

We addressed the issue of occupational retention by estimating the impacts of the JSA treatments on the likelihood that claimants found new jobs in the same occupations as their old jobs.

TABLE VIII.4

POST-UI OCCUPATIONAL DISTRIBUTIONS BY TREATMENT AND CONTROL GROUP
(Percent)

Occupation	Control Group	SJSA Group	Combined IJSA and IJSA+ Group
	DISTRICT OF COLUMBIA		
Professional	14.4	16.9	12.8
Technical & Sales	1.0	2.1	3.4
Clerical	44.1	41.2	40.2
Service	23.3	23.5	28.6
Crafts	7.9	9.9	6.8
Operator	1.5	1.2	1.7
Laborer	6.9	4.1	6.4
Sample Size	202	243	234
	FLORIDA		
Professional	13.4	14.6	13.0
Technical & Sales	18.9	20.0	16.5
Clerical	26.0	25.6	26.5
Service	14.8	13.4	14.6
Crafts	8.5	10.1	12.3
Operator	13.4	12.6	12.5
Laborer	3.2	3.5	3.2
Sample Size	411	454	431

SOURCE: JSA demonstration follow-up survey.

Table VIII.5 shows that in D.C., there was not much of an impact on occupational retention. SJSA decreased the probability of claimants remaining in the same occupation by 2.5 percentage points, but this impact was not statistically significant. In Florida, the impact was also negative, with SJSA decreasing the probability of claimants obtaining a job in the same occupation by 1.5 percentage points, but again the impact was statistically insignificant. This absence of substantial change in the occupational distribution in conjunction with the absence of an impact on the retention rate leads us to conclude the SJSA treatment did not substantially affect the occupational focus of job search activities.

Table VIII.5 presents the impacts of the combined individualized treatments on occupational retention rates. According to these results, the combined treatments did not have a substantial impact on the retention rate in either D.C. or Florida.

TABLE VIII.5

ESTIMATED IMPACTS OF TREATMENTS ON PROBABILITY OF OCCUPATIONAL RETENTION
(Percentage Points)

Treatment	District of Columbia	Florida
SJSA Impact	-2.5	-1.5
Combined IJSA and IJSA+ Impact	1.2	1.4
Control Group Mean (Percent)	78.4	62.9
Sample Size	730	1,294

SOURCE: JSA demonstration follow-up survey.

NOTE: Impacts shown are the marginal effects of the treatments calculated based on logit regression estimates. Statistical significance results are based on hypothesis tests of the logit coefficients.

TABLE VIII.5

ESTIMATED IMPACTS ON PROBABILITY OF OCCUPATION RETENTION
(Percentage Points)

Treatment	District of Columbia	Florida
SJSA Impact	-2.5	-4.5
IJSA Impact	-0.6	-4.2
IJSA+ Impact	3.4	4.2
Combined IJSA and IJSA+ Impact	1.2	0.6
Control Group Mean (Percent)	78.4	65.9
Sample Size	730	1,294

SOURCE: JSA demonstration follow-up survey.

NOTE: Impacts shown are the marginal effects of the treatments calculated based on logit regression estimates. Statistical significance results are based on hypothesis tests of the logit coefficients.

IX. IMPACTS ON JOB SEARCH ACTIVITIES

Improving the skill and aggressiveness with which claimants search for jobs is a key objective of the JSA demonstration. Indeed, the improvement of job search efforts, which in turn leads to more rapid re-employment and a better "match" between job seekers' skills and the jobs they ultimately land, can be viewed as the direct objective of the JSA demonstration. To the extent the JSA demonstration works the way it is intended, changes in UI receipt, employment, and earnings are the ultimate results of the demonstration's intensified job search activities.

In previous chapters, we have noted that the JSA treatments decreased UI receipt and increased employment-related outcomes. This finding provides some indication that the treatments may have increased the intensity of claimants' job search efforts. But since findings presented in earlier chapters also suggest that the effects of JSA on UI receipt were at least partly due to the increased enforcement of UI eligibility rules, it is possible that the impacts on UI receipt are not attributable to an increase in search intensity. To investigate this issue in greater detail, we estimated the impacts of the JSA treatments on job search behavior using measures of job search activities drawn from the follow-up survey.

The estimates presented in this chapter provide some evidence that the JSA demonstration encouraged more aggressive job searches. More specifically, in both D.C. and Florida, the structured treatment (SJSA) led claimants to contact more prospective employers per week. In D.C., the individualized treatments (IJSA and IJSA+) increased both the likelihood that claimants searched for employment following their job loss and the number of employer contacts per week. In Florida, the individualized treatments increased both contacts and hours of search per week. The other estimated impacts on job search were less consistent and less precise.

Another potential impact of the demonstration was to increase claimants' use of the state Job Service (JS) to assist in their job search. We find that the treatments consistently increased the likelihood that claimants used their state JS. This is not surprising given that the treatments were specifically designed to encourage claimants to use the JS. The treatments also tended to increase the referrals that claimants received from the JS. However, there was no evidence of impacts on the likelihood of obtaining job offers through JS referrals. Thus, it appears that the JSA demonstration was successful in getting claimants to use the state JS, but less successful in translating these services into employment opportunities.

A. IMPACTS ON JOB SEARCH EFFORT

We examined job search effort using three measures that were created from claimant responses to the follow-up survey. The three measures are (1) whether claimants searched for employment after their job loss, (2) hours per week spent searching for work, and (3) number of employers contacted per week. We estimated impacts on all of these measures by calculating the difference between mean outcomes for the treatment and control groups.[1] When we investigated survey-based job characteristic measures in the previous chapters, we combined the individualized treatments to create reasonable sample sizes. In this chapter, our sample sizes are larger than in the previous chapter, since we have job search measures for all survey respondents rather than just employed respondents. Given these larger sample sizes, we present estimated impacts separately for IJSA and IJSA+, as well as estimated impacts for IJSA and IJSA+ combined.

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[1]Since these estimates are based on the survey, they are subject to potential survey nonresponse bias. In an effort to control for nonresponse bias as much as possible, we generated alternative estimates based on regressions that control for a variety of individual factors as determinants of the job search measures. We found that these regression estimates were similar to the treatment-control differences and therefore chose not to present them.

1. Structured Job Search Assistance

SJSA may have had some positive effect on the job search effort of claimants, but the evidence is not consistently strong. For the three measures that we report on in Table IX.1, the only sizable increases in job search effort occurred for the number of employer contacts per week. SJSA increased contacts in D.C. and Florida by an estimated 1.6 and 1.4 contacts per week, respectively. These statistically significant estimates represent roughly a 10 percent increase in contacts for the average claimant. The estimated impacts on the other measures provide no clear evidence of increased search effort: SJSA had no statistically significant effect on the likelihood that claimants searched for work or the hours per week they spent searching.

Given that SJSA had only modest effects on employment and earnings, as discussed in Chapter VII, we are not surprised to find that it had a modest impact on search effort. SJSA could have also potentially generated more effective job searches without increasing effort. For example, claimants who attended the job search workshop may have learned how to target their efforts more efficiently or communicate more effectively with potential employers. Evaluating the effectiveness of a claimant's job search, holding effort constant, would require a more detailed investigation and is beyond the scope of this study. However, the modest impacts of SJSA on employment and earnings (described in Chapter VII) suggest that any impacts on search effectiveness were limited.

2. Individualized Job Search Assistance Treatments

Among the individualized job search assistance treatments, the strongest evidence of positive impacts on effort occurred for IJSA+ in Florida. Estimates presented in Table IX.2 show that IJSA+ increased the likelihood that claimants searched for work by 3 percentage points, increased average hours of search by 1.7 hours per week, and increased contacts by 2.1 contacts per week. All of these estimates are statistically significant at least at the 95 percent level of confidence. The estimated

TABLE IX.1

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE
ON JOB SEARCH EFFORT

Job Search Measure	District of Columbia			Florida		
	Control Group Mean	SJSA Group Mean	Estimated Impact	Control Group Mean	SJSA Group Mean	Estimated Impact
Percent Who Searched for Employment	89.5	89.9	0.4	95.5	94.9	-0.6
	(1.5)	(1.5)	(2.1)	(0.8)	(0.8)	(1.2)
Hours Per Week Searching for Employment	14.3	14.5	0.2	14.8	15.2	0.4
	(0.6)	(0.6)	(0.8)	(0.5)	(0.4)	(0.6)
Contacts Per Week	13.1	14.7	1.6*	13.2	14.5	1.4**
	(0.7)	(0.9)	(1.2)	(0.5)	(0.6)	(0.7)
Sample Size	419	425		640	703	

SOURCE: JSA demonstration follow-up survey.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

*Statistically significant at the 90 percent level of confidence in a one-tailed test.
**Statistically significant at the 95 percent level of confidence in a one-tailed test.

TABLE IX.2

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE
ON JOB SEARCH EFFORT

		IJSA		IJSA +		Combined IJSA Treatments	
Job Search Measure	Control Group Mean	Treatment Group Mean	Estimated Impact	Treatment Group Mean	Estimated Impact	Combined Treatment Group Mean	Estimated Impact
Percent Who Searched for Employment	89.5	94.3	4.8**	90.2	0.7	92.4	2.9*
	(1.5)	(1.5)	(2.1)	(2.1)	(2.6)	(1.3)	(2.0)
Hours Per Week Searching for Employment	14.3	14.9	0.6	15.2	0.9	15.0	0.7
	(0.6)	(0.7)	(0.9)	(0.8)	(1.0)	(0.5)	(0.8)
Contacts Per Week	13.1	15.0	1.9*	16.1	3.0*	15.5	2.4**
	(0.7)	(0.9)	(1.2)	(1.7)	(1.9)	(0.9)	(1.2)
Sample Size	419	228		194			
Percent Who Searched for Employment	95.5	94.9	-0.5	98.4	3.0***	96.6	1.1
	(0.8)	(1.2)	(1.4)	(0.7)	(1.1)	(0.7)	(1.1)
Hours Per Week Searching for Employment	14.8	15.5	0.7	16.4	1.7**	15.9	1.2**
	(0.5)	(0.6)	(0.7)	(0.7)	(0.8)	(0.4)	(0.6)
Contacts Per Week	13.2	14.6	1.5*	15.3	2.1**	15.0	1.8**
	(0.5)	(0.9)	(1.0)	(1.0)	(1.1)	(0.7)	(0.8)
Sample Size	640	355		321		676	

SOURCE: JSA demonstration follow-up survey.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

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impacts on search hours and employer contacts per week represent increases of more than 10 percent above the control group means.

For the other treatments, IJSA in Florida and IJSA and IJSA+ in D.C., the evidence is somewhat less clear that search intensity increased in response to the services. In D.C., IJSA increased the claimants who reported they searched for work by 4.8 percentage points, and this impact is significant at the 95 percent confidence level. The estimates presented in Table IX.2 also imply that in D.C. the IJSA and IJSA+ treatments increased average employer contacts per week by 1.9 and 3.0, respectively. These impacts are substantial, representing a 15 to 20 percent increase in contacts, and statistically significant at the 90 percent confidence level. Similarly, the estimates for IJSA in Florida suggest that the treatment generated 1.5 extra employer contacts per week, on average.

The lack of consistently significant impacts in Table IX.2 is due partly to the small size of these samples. In the case of the IJSA and IJSA+ treatments, samples are especially small because of the survey's design. Our goal in the survey was to interview the IJSA and IJSA+ groups at half the rate of the SJSA and control groups so the IJSA and IJSA+ respondents could be combined for the data analysis into a single group comparable in size to the SJSA or control group respondents. This is reflected in the sample sizes shown in Table IX.2, which show that in Florida, for example, we have survey responses from 640 control group members, compared with 355 IJSA group members and 321 IJSA+ group members.

Given the similarity in the services offered to and received by IJSA and IJSA+ claimants, we combined these groups in our analysis to increase our statistical power in evaluating the impacts of the IJSA treatments. The final column in Table IX.2 shows the estimated impacts based on these combined groups. The combined IJSA treatments generated 2.4 extra employer contacts per week in D.C. and 1.8 extra contacts in Florida. Both of these estimates are statistically significant at the

95 percent level. The combined IJSA treatments also generated a statistically significant increase in the percentage of D.C. claimants who searched for employment and in the average time Florida claimants spent searching per week. The remaining estimated impacts are all positive but not statistically significant.

B. IMPACTS ON CONTACTS WITH STATE EMPLOYMENT SERVICE

One of the JSA demonstration's main strategies to promote more effective job searches was to increase use of the state JS. In this section, we explore the impact of the JSA treatments on claimants' experiences with the state JS. Our analysis focuses on three measures: (1) whether claimants reported contact with the JS, (2) whether claimants received job referrals through the JS, and (3) whether claimants received job offers based on referrals from the JS.

1. Structured Job Search Assistance

SJSA increased reported contact with the JS in both states. This is not surprising because SJSA required that claimants participate in services offered through the JS. In D.C., the rate at which claimants reported contact with the JS increased by 10.0 percentage points in response to SJSA, while the corresponding increase in Florida was about 6.3 percentage points. Both of these estimates are statistically significant at the 99 percent confidence level.

One interesting point, based on Table IX.3, is that although the treatment groups had higher rates of JS contact, a high proportion of control group members also reported JS contact. In D.C., 44.9 percent of the control group reported contact with the JS, while in Florida, 70.9 percent of the control group reported similar contact. These high rates among control groups in both states are not surprising given that both states require most new claimants to at least register with the JS.

TABLE IX.3

ESTIMATED IMPACTS OF STRUCTURED JOB SEARCH ASSISTANCE ON USE OF STATE JOB SERVICE

Measure of Job Service Use	District of Columbia			Florida		
	Control Group Mean	SJSA Group Mean	Estimated Impact	Control Group Mean	SJSA Group Mean	Estimated Impact
Percent Who Contacted State Job Service	44.9	54.8	10.0***	70.9	77.2	6.3***
	(2.4)	(2.4)	(3.4)	(1.8)	(1.6)	(2.4)
Percent Who Received Job Referrals from State Job Service	15.5	24.2	8.7***	28.9	32.3	3.4*
	(1.8)	(2.1)	(2.7)	(1.8)	(1.8)	(2.5)
Percent Who Received Job Offers Based on Referrals	2.9	2.8	0.0	7.3	6.0	-1.4
	(0.8)	(0.8)	(1.1)	(1.0)	(0.9)	(1.4)
Sample Size	419	425		640	703	

SOURCE: JSA demonstration follow-up survey.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

*Statistically significant at the 90 percent level of confidence in a one-tailed test.
***Statistically significant at the 99 percent level of confidence in a one-tailed test.

Furthermore, the particularly high rate in Florida is also not surprising given that the JS and UI offices in that state have been merged into the new Jobs and Benefits offices.

In addition to increased contact with the JS, the SJSA treatment also increased the rate at which claimants received job referrals from the JS. In D.C., 24.2 percent of the SJSA treatment group received a job referral from the JS. This represents an increase of 8.7 percentage points when compared with the rate for the control group (15.5 percent), and this increase is statistically significant at the 99 percent confidence level. The SJSA group also had a higher rate of job referrals than the control group in Florida--an increase of 3.4 percentage points, which represents more than 10 percent of the control group mean.

Although SJSA increased contact with the JS and job referrals from the JS, the treatment had no impact on the percentage of claimants who received job offers based on the JS referrals. On the contrary, the two estimated impacts of SJSA on the probability that a JS referral led to a job offer were zero or negative. This finding suggests that increased contact with the JS did not bring about greater re-employment that could be attributed directly to JS referrals. However, the increased contact with JS may still have been indirectly responsible for greater re-employment. Claimants could have used their contacts with JS to build job search networks that offered job opportunities outside JS referrals. Earlier chapters provide some evidence that the SJSA treatment had a positive impact on re-employment, but it is hard to know what role specific JS services or referrals played in that impact.

2. Individualized Job Search Assistance

The IJSA and IJSA+ treatments had impacts on JS contacts that were similar to the impacts of SJSA. Estimates presented in Table IX.4 show that IJSA increased the reported rate of contact with

TABLE IX.4

ESTIMATED IMPACTS OF INDIVIDUALIZED JOB SEARCH ASSISTANCE
ON USE OF STATE JOB SERVICE

Measure of Job Service Use	Control Group Mean	IJSA Treatment Group Mean	IJSA Estimated Impact	IJSA + Treatment Group Mean	IJSA + Estimated Impact	Combined IJSA Treatments Combined Treatment Group Mean	Combined IJSA Treatments Estimated Impact
Percent Who Contacted State Job Service	44.9 (2.4)	58.8 (3.3)	13.9*** (4.1)	52.1 (3.6)	7.2** (4.3)	55.7 (2.4)	10.8*** (3.4)
Percent Who Received Job Referrals from State Job Service	15.5 (1.8)	18.4 (2.6)	2.9 (3.1)	24.2 (3.1)	8.7*** (3.6)	21.1 (2.0)	5.6** (2.7)
Percent Who Received Job Offers Based on Referrals	2.9 (0.8)	2.2 (0.9)	-0.1 (1.3)	1.5 (0.9)	-1.3 (1.2)	1.9 (0.7)	-1.0 (1.1)
Sample Size	419	228		194		422	
Percent Who Contacted State Job Service	70.9 (1.8)	76.9 (2.2)	6.0** (2.9)	74.1 (2.4)	3.2 (3.0)	75.6 (1.7)	4.7** (2.4)
Percent Who Received Job Referrals from State Job Service	28.9 (1.8)	32.7 (2.5)	3.8 (3.1)	39.3 (2.7)	10.3*** (3.3)	35.8 (1.8)	6.9*** (2.6)
Percent Who Received Job Offers Based on Referrals	7.3 (1.0)	5.1 (1.2)	-2.3 (1.6)	8.1 (1.5)	0.8 (1.8)	6.5 (0.9)	-0.8 (1.4)
Sample Size	640	355		321		676	

SOURCE: JSA demonstration follow-up survey.

NOTE: The impact estimates are based on differences in outcomes between the control group and the treatment groups.

*Statistically significant at the 90 percent confidence level in a one-tailed test.
**Statistically significant at the 95 percent confidence level in a one-tailed test.
***Statistically significant at the 99 percent confidence level in a one-tailed test.

the JS in both states, while IJSA+ increased the rate of contact in D.C. The estimated effect of IJSA+ in Florida was also positive but not statistically significant.

The individualized treatments also tended to increase job referrals from JS. In both states, the IJSA+ treatment generated large and statistically significant increases in the job referrals from the JS. In D.C., IJSA+ increased the probability of receiving a job referral by 8.7 percentage points, to 24.2 percent for the IJSA+ group compared with 15.5 percent for the control group. The impact of IJSA+ was similarly large in Florida, where the increase was equal to 10.3 percentage points, from 28.9 percent for the control group to 39.3 percent for the IJSA+ group.

The estimated impacts of IJSA on referrals were also positive but not statistically significant in either state. In D.C., the rate of job referrals from the JS was 18.4 percent for the IJSA group compared with 15.5 percent for the control group, a statistically insignificant difference of only 2.9 percentage points. Similarly in Florida, the referral rate from the JS was 32.7 percent for the IJSA group compared with 28.9 percent for the control group, a statistically insignificant difference of 3.8 percentage points.

Despite the increased referrals generated by the IJSA and IJSA+ treatments, the treatments did not increase the job offers based on these referrals. The estimated impact of IJSA on job offers was negative and statistically insignificant in both states. The estimate for IJSA+ was negative and statistically insignificant in D.C. and positive but still statistically insignificant in Florida.

These findings suggest that the individualized JSA treatments achieved the objective of increasing access to the JS and the use of JS referrals. However, achieving these objectives did not also generate increased job offers. Estimates based on the combined individualized JSA treatments, which are shown in the final column of Table IX.4, reinforce this conclusion. The combined individualized JSA treatments increased contact with JS and job referrals from JS in both states. But

the combined treatments did not increase job offers based on JI referrals--the estimates in both cases are negative and statistically insignificant.

X. THE COST-EFFECTIVENESS OF THE JSA TREATMENTS

In this chapter, we combine estimates of the impacts of the JSA demonstration with cost estimates to assess whether the benefits of each of the three JSA treatments exceeded its costs. We assess the benefits and costs of each treatment from several different perspectives. For each treatment, we compute the net benefits per claimant (benefits minus costs divided by the number of claimants) and the rate of return per dollar spent (benefits minus costs divided by costs) to claimants, DOL, the entire government, and society as a whole. The net benefit and rate of return estimates in this chapter measure the cost-effectiveness of different JSA treatments, and can help policymakers determine the desirability of the treatments.

The cost-effectiveness analysis in this chapter uses estimates from earlier chapters on the costs of operating the demonstration and providing JSA (Chapter IV), the impacts of each treatment on UI receipt (Chapter V), and the impacts on employment outcomes (Chapter VI). For each treatment, Chapter IV provides estimates of the costs of the demonstration and JSA services; Chapter V provides estimates of the reduction in UI payments that benefits DOL; and Chapter VI provides estimates of the earnings increases that benefit claimants. In this chapter, we weigh the benefits against the costs to determine the extent to which the three JSA treatments were cost-effective in D.C. and Florida.

Based on the results presented in this chapter, we come to the following conclusions:

- **From the perspective of DOL-sponsored programs, none of the treatments were cost-effective in either D.C. or Florida.** The reductions in UI payments never outweighed the costs of the demonstration. Furthermore, most of the additional tax revenue went to other government agencies or departments.

- **For government as a whole and society (i.e. government and UI claimants together), the treatments were cost-effective in D.C. but not in Florida.** These results are largely driven by positive earnings impacts in D.C. and zero or negative earnings impacts in Florida. The earnings gains by D.C. claimants provide benefits to the claimants themselves *and* to government through an increase in the tax base.

- **The results cannot conclusively identify which treatment is most cost-effective.**

A. IDENTIFYING THE BENEFITS AND COSTS

Our analysis of benefits and costs focuses on three parties who were affected by the three JSA treatments:

- UI claimants
- DOL (SESA, which encompasses the UI system and the JS)
- Government as a whole, including DOL

After analyzing the benefits to each of the three parties separately, we consider all three parties together to evaluate the cost-effectiveness of each JSA treatment from the perspective of society.

1. UI Claimants

The JSA treatments were designed to hasten the reemployment of UI claimants. The negative impacts on the duration and amount of UI receipt (Chapter V) and positive impacts on employment and earnings (Chapter VII) suggest that the treatments were modestly successful in moving UI claimants from UI to employment. The impacts on UI receipt and earnings had opposite effects on the incomes of UI claimants:

- Larger earnings raised claimants' incomes
- Smaller UI payments lowered claimants' incomes

Whether claimants' incomes rose or fell depended on the relative sizes of these two effects, which we estimate in section B.

The increase in pre-tax earnings induced by the JSA treatments does not fully account for the benefits of increased employment to UI claimants. Government taxes capture part of each additional dollar of earnings. We impute the additional income taxes (state and federal), FICA taxes and UI taxes paid by UI claimants for each additional dollar of earnings. Adding together the federal income tax rate for the lowest income bracket (15 percent), the state income tax rates (6 percent in D.C. and 0 percent in Florida), the FICA tax rate (approximately 8 percent), and the UI tax rate (approximately 1 percent in each state) leads to tax rates of 30 percent in D.C. and 24 percent in Florida.[1] Therefore, we assume that for each additional dollar of earnings, UI claimants pay additional taxes of $.30 in D.C. and $.24 in Florida.[2]

Furthermore, employed people often receive fringe benefits, and the generosity of these benefits is typically higher in highly paid jobs. Therefore, we impute the value of the fringe benefits likely to accompany each additional dollar of earnings from aggregate figures provided by the Bureau of Labor Statistics (BLS).[3] According to the BLS, the average cost of employing a civilian was $20.29

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[1]The UI tax rates faced by employers are fairly complicated. Therefore, we use the *average* UI tax rate (total tax payments divided by total wages in covered employment) in each state as an estimate of the UI tax rate. The average tax rate is slightly less than 1 percent in Florida and slightly more than 1 percent in D.C. For simplicity, we assume the UI tax rate in both states is 1 percent.

[2]Other reasonable ways exist to impute the additional tax revenue generated by the JSA treatments. However, as shown in this chapter, the conclusions resulting from our net benefit analysis are not influenced by seemingly large changes in the tax rates faced by claimants.

[3]In Chapter VIII, we show that the treatments in D.C. had a positive impact on the proportion of employed exclaimants with jobs that provide health insurance and pension benefits. However, we lack measures of the

(continued...)

in March 1999, of which $14.72 was paid in wages.[4] These figures suggest that fringe benefits equal approximately 38 percent of earnings. Therefore, we assume that each additional dollar of earnings is accompanied by additional fringe benefits worth $.38.

2. DOL

DOL programs were affected financially by three factors related to the costs and impacts of the JSA treatments:

- The costs of operating the demonstration and providing JSA (Chapter IV)
- Reduced payments to UI claimants (Chapter V)
- Increased tax revenue from UI taxes

The costs of operating the demonstration and providing JSA were documented in Chapter IV. The reduction in UI payments due to the treatments constitutes a cost to UI claimants but a benefit to the UI system that makes the payments. Lastly, the treatment-induced increase in earnings documented in Chapter VII led to an increase in UI taxes collected, which benefits DOL. The reduction in UI payments and increase in UI taxes constitute transfers from UI claimants to DOL programs. These benefits to DOL are measured in section B.

3. Government as a Whole

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[3](...continued)
average value of each of these two benefits. Furthermore, fringe benefits other than health insurance and pension benefits accompany good jobs, and our survey cannot capture those benefits. Therefore, we use the aggregate BLS figure to impute the total value of all fringes likely to accompany each additional dollar of earnings.

[4]News release by the US Department of Labor, "Employer Costs for Employee Compensation - March 1999," USDL: 99-173, June 24, 1999.

The benefits and costs to government as a whole include the benefits and costs to UI system, but include additional two additional benefits:

- Increased tax revenue from income taxes (both state and federal)
- Increased tax revenue from FICA (i.e. Social Security taxes)

The increase in income and FICA tax receipt constitute transfers from UI claimants to the government, and these transfers are measured in section B.

B. BENEFITS, COSTS AND COST-EFFECTIVENESS OF JSA TREATMENTS

In this section, we estimate the net benefit to UI claimants, DOL, the entire government including DOL, and society from each of the three JSA treatments for both states. The results are provided in Tables X.1 and X.2 for D.C. and Florida, respectively. Positive numbers indicate benefits and negative numbers indicate costs. The net benefit per claimant from each treatment is computed by subtracting the per-claimant costs from the per-claimant benefits. The rate of return is computed by subtracting the costs from the benefits, and then dividing by the demonstration costs. Therefore, the net benefit measures the return for each treated claimant; the rate of return measures the return for each dollar spent. Our two measures of cost-effectiveness--net benefits and the rate of return--are positive when the benefits outweigh the costs and negative when the costs outweigh the benefits.

While the costs of the intervention were all incurred in the intake year (June 1995 to June 1996 in D.C. and March 1995 to March 1996 in Florida), some benefits from higher earnings and lower UI

receipt accrued in later years.[5] We discount benefits that accrued after the first year using an annual interest rate of 5 percent.[6]

1. Cost-Effectiveness of JSA in D.C.

In this section, we show that the positive impacts on earnings ensured that all three JSA treatments in D.C. were cost-effective from the perspective of both claimants and government as a whole. However, from DOL's perspective, the JSA treatments were not cost-effective because the demonstration costs exceeded the estimated reduction in UI payments for each treatment.

a. D.C. Claimants

As shown in Table X.1, UI claimants in D.C. seem to have benefitted from each of the three treatments, with SJSA yielding the largest net benefit. The estimated net benefits from all three JSA treatments to D.C. claimants were generated by the positive earnings impacts documented in Chapter VII. The present discounted value of the quarterly earnings impacts (relative to the control group) over the first 10 quarters is $1,921 per SJSA participant, $1,126 per IJSA participant, and $769 per IJSA+ participant. The increase in fringe benefits approximately compensated members of each treatment group for increased tax payments and reduced UI payments. Therefore, the net per-capita benefits approximately equal the earnings impacts: $1,930 per claimant in SJSA, $1,136 per claimant in IJSA, and $806 per claimant in IJSA+. These net benefits suggest that each treatment

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[5]Our data allowed us to measure the impacts of the JSA treatments on UI benefits for two years following enrollment in the demonstration, and to measure the impacts on earnings for 10 quarters in D.C. and 12 quarters in Florida.

[6]We do not explicitly account for inflation in computing net benefits. However, because our conclusions are not sensitive to even large increases in the interest rate, and because increases in the inflation rate would influence the net benefit computations in exactly the same manner as increases in the interest rate, omitting the inflation rate is inconsequential.

TABLE X.1

COST-EFFECTIVENESS OF JSA TREATMENTS IN DC
(Dollars per Claimant)

	Perspective				
		Government			
Benefit and Costs	Claimant	DOL	Other Government	Government Total	Society
		IJSA			
Earnings	1,921	0	0	0	1,921
Fringe Benefits	726	0	0	0	726
Claimants' Tax Payments	-576	19	557	576	0
UI Payments	-141	141	0	141	0
Demonstration Costs					
Local Office Costs	0	-251	0	-251	-251
Central Office Costs	0	-35	0	-35	-35
Net Benefits	**1,930**	**-126**	**557**	**431**	**2,361**
Rate of Return	**NA**	**-44%**	**NA**	**151%**	**826%**
		IJSA			
Earnings	1,126	0	0	0	1,126
Fringe Benefits	426	0	0	0	426
Claimants' Tax Payments	-338	11	327	338	0
UI Payments	-78	78	0	78	0
Demonstration Costs					
Local Office Costs	0	-164	0	-164	-164
Central Office Costs	0	-35	0	-35	-35
Net Benefits	**1,136**	**-110**	**327**	**217**	**1,353**
Rate of Return	**NA**	**-55%**	**NA**	**109%**	**680%**
		IJSA+			
Earnings	769	0	0	0	769
Fringe Benefits	291	0	0	0	291
Claimants' Tax Payments	-231	8	223	231	0
UI Payments	-23	23	0	23	0
Demonstration Costs					
Local Office Costs	0	-181	0	-181	-181
Central Office Costs	0	-35	0	-35	-35
Net Benefits	**806**	**-186**	**223**	**37**	**844**
Rate of Return	**NA**	**-86%**	**NA**	**17%**	**391%**

in D.C. was cost-effective from the claimants' perspective. Furthermore, our confidence that the three treatments yielded net benefits to claimants is bolstered by that fact that our positive net benefit estimates are not driven by the assumptions that we make about the interest rate, the relationship between earnings and fringe benefits, or tax rates.[7]

b. DOL

Our estimates suggest that none of the three treatments in D.C. were cost-effective from DOL's perspective. As indicated in Chapter IV, the estimated cost per claimant was $286 for SJSA, $199 for IJSA, and $216 for IJSA+. In the first two years, the amount recovered from reduced UI payments and increased UI tax payments was smaller than the cost for each of the three treatments. On net, all three treatments were costly to DOL: the net costs were $126 per claimant in SJSA, $110 per claimant in IJSA, and $186 per claimant in IJSA+. These net costs correspond to the following rates of return on the investment of funds in the demonstration:

- -44 percent for SJSA
- -55 percent for IJSA
- -86 percent for IJSA+

[7]The year-one impacts on earnings alone were enough to compensate UI claimants for reduced UI payments resulting from all three treatments. Therefore, assuming a much higher interest rate, which would lead us to more heavily discount earnings impacts after year one, would not change our conclusion. Furthermore, even if the three treatments had *no* impact on fringe benefits (despite the large impacts on quarterly earnings), the net benefit estimates for all three treatments would still be positive. Lastly, even a tax rate of 100 percent would not reverse our conclusions because for all three treatments, the additional fringe benefits more than compensated UI claimants for reduced UI payments.

These rates of return suggest that each dollar invested led to *less than* one dollar in benefits. Put differently, part of the investment in the JSA treatments was lost because the benefits to DOL from reduced UI payments and additional tax revenue were not large enough to cover the costs. For example, in the case of SJSA, our estimates suggest that $.44 of each dollar invested was lost. The analogous losses for IJSA and IJSA+ were $.55 and $.86, respectively. Therefore, none of the treatments were cost-effective from DOL's perspective.

The cost-effectiveness of each treatment depends on its costs, which are difficult to measure precisely. However, our analysis suggests that the magnitude of the measurement errors would need to be very large to change our conclusion that none of three JSA treatments were cost-effective for DOL.[8]

c. Government

The JSA treatments in D.C. were cost-effective for government as a whole. Because the UI tax rate is small, the benefits to DOL from increased earnings are very small. However, the *cumulative* tax rate, computed by adding together the tax rates from all government taxes on earnings (federal income, state income, FICA, and UI) is quite large. Therefore, the large estimated impacts of JSA treatments on earnings in D.C. led to positive net benefits to government: $431 per claimant in SJSA, $217 per claimant in IJSA, and $37 per claimant in IJSA+. These net benefits correspond to the following positive rates of return:

- 151 percent for SJSA
- 109 percent for IJSA
- 17 percent for IJSA+

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[8]Our estimate of the costs of SJSA, for example, would have to overestimate the true costs by almost 80 percent before we could conclude that SJSA yielded benefits to DOL.

One dollar invested in SJSA, for example, generated $1.51 in benefits for government as a whole. We conclude that SJSA and IJSA were cost-effective to government as a whole, and that IJSA+ was approximately budget neutral.

This conclusion is not driven by errors in estimating tax rates. If our estimate of the cumulative tax rate is too large, then the net benefits to government of IJSA+--which produced the smallest benefits to government according to Table X.1--may actually be negative.[9] However, the cumulative tax rate in D.C. must be *at least* as large as the sum of the federal income tax rate for the lowest tax bracket (15 percent), the state income tax rate (6 percent), and the FICA tax rate (approximately 8 percent). Furthermore, some additional analysis suggests that our conclusions are not driven by errors in estimating demonstration costs.[10]

d. Society

Finally, we conclude that the three JSA treatments were cost-effective from the perspective of society. We estimated the benefits to society by summing together the benefits to total government and the benefits to claimants, and likewise for the costs. Because the treatments were cost-effective from the perspective of both claimants and government as a whole, they were cost-effective from the perspective of society. The net benefits to society were positive for all three treatments: $2,361 per claimant in SJSA, $1,353 per claimant in IJSA, and $844 per claimant in IJSA+. These net benefits correspond to the following rates of return:

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[9]For example, if the cumulative tax rate in D.C. were 24 percent instead of 30 percent, the net benefit per IJSA+ participant would be -$1.

[10]If we underestimated all program costs by 20 percent, the net benefits would be $360, $167 and -$17 for SJSA, IJSA and IJSA+, respectively, suggesting net costs to IJSA+. However, we would still conclude that IJSA+ was approximately budget neutral, and that the net benefits to government from SJSA and IJSA in D.C. were positive--even if we underestimated the treatment costs by 20 percent.

- 826 percent for SJSA
- 680 percent for IJSA
- 391 percent for IJSA+

For example, each dollar invested in SJSA led to benefits equal to that dollar plus an additional $8.26. In other words, from society's perspective, the treatments "paid for themselves" through higher earnings and other benefits.

2. Cost-Effectiveness of JSA in Florida

As in D.C., we estimated the cost-effectiveness of each JSA treatment in Florida. In this section, we show that none of the treatments in Florida were cost-effective for claimants or government because they failed to raise earnings.

a. Florida Claimants

Based on Table X.2, we conclude that SJSA and IJSA were costly to claimants in Florida, and that IJSA+ generated benefits that approximately equaled the costs to claimants. Unlike in D.C., there is no evidence that JSA treatments in Florida had positive impacts on earnings. However, as in D.C., the treatments generally reduced UI payments to claimants (at least in the first year for the individualized treatments). Because reductions in UI payments were not compensated by increases in earnings, all three treatments imposed net costs on UI claimants: $653 per claimant in SJSA, $196 per claimant in IJSA, and $12 per claimant in IJSA+. Furthermore, none of the assumptions that we made about the interest rate, the relationship between earnings and fringe benefits, or tax

TABLE X.2

COST-EFFECTIVENESS OF JSA TREATMENTS IN FLORIDA
(Dollars per Claimant)

	Perspective				
		Government			
Benefit and Costs	Claimant	DOL	Other Government	Government Total	Society
SJSA					
Earnings	-554	0	0	0	-554
Fringe Benefits	-209	0	0	0	-209
Claimants' Tax Payments	133	-6	-127	-133	0
UI Payments	-23	23	0	23	0
Demonstration Costs					
Local Office Costs	0	-205	0	-205	-205
Central Office Costs	0	-36	0	-36	-36
Net Benefits	**-653**	**-224**	**-127**	**-351**	**-1,004**
Rate of Return	**NA**	**-93%**	**NA**	**-146%**	**-416%**
DJSA					
Earnings	-86	0	0	0	-86
Fringe Benefits	-33	0	0	0	-33
Claimants' Tax Payments	21	-1	-20	-21	0
UI Payments	-98	98	0	98	0
Demonstration Costs					
Local Office Costs	0	-61	0	-61	-61
Central Office Costs	0	-36	0	-36	-36
Net Benefits	**-196**	**0**	**-20**	**-20**	**-215**
Rate of Return	**NA**	**0%**	**NA**	**-20%**	**-222%**
UJSA					
Earnings	31	0	0	0	31
Fringe Benefits	12	0	0	0	12
Claimants' Tax Payments	-7	0	7	7	0
UI Payments	-47	47	0	47	0
Demonstration Costs					
Local Office Costs	0	-67	0	-67	-67
Central Office Costs	0	-36	0	-36	-36
Net Benefits	**-12**	**-55**	**7**	**-48**	**-61**
Rate of Return	**NA**	**-54%**	**NA**	**-47%**	**-59%**

rates influence the our conclusion that the treatments imposed net costs on claimants.[11]

b. DOL

The estimated treatment-induced reductions in UI payments were too small to compensate DOL for the costs of SJSA and IJSA+ in Florida. The net per-capita costs were $224 per claimant in SJSA and $55 per claimant in IJSA+. For IJSA, the reduction in UI payments of $98 per claimant exactly compensated DOL for the per-capita treatment cost of $97 (plus $1 of lost tax revenue resulting from lower earnings). The rates of return for the three treatments are given below:

- -93 percent for SJSA
- 0 percent for IJSA
- -54 percent for IJSA+

DOL lost $.93 of every dollar invested in SJSA, $.54 of every dollar invested in IJSA+, and "broke even" on their investments in IJSA. As in D.C., we conclude that the JSA treatments in Florida were not cost-effective for DOL.

Net benefits and rates of return depend on the costs of the treatments, which are difficult to measure precisely. However, our conclusion--that the JSA treatments in Florida were not cost-effective for DOL--does not rely on having perfect cost estimates.[12]

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[11] Our estimate of the present discounted value of earnings is not very sensitive to changes in the interest rate because we have only 2 to 3 years of earnings data. Furthermore, even if the three treatments had no impact on fringe benefits, our net benefit estimates would still all be negative. Lastly, no tax rate up to 100 percent would reverse our conclusion that the treatments failed to produce net benefits for claimants in Florida.

[12] If we have overestimated the cost of IJSA, for example, then the net benefits of this treatment to DOL may actually be positive. Regardless, both IJSA and IJSA+ were approximately budget neutral to UI. For SJSA, the (continued...)

c. **Government**

Our calculations suggest that none of the treatments in Florida were cost-effective from the perspective of government as a whole. Because the treatments failed to raise earnings in Florida, the government received no additional tax revenue from treatment group members. The estimated net costs to government were $351 per claimant in SJSA, $20 per claimant in IJSA, and $40 per claimant in IJSA+. The corresponding rates of return are given below:

- -146 percent for SJSA
- -20 percent for IJSA
- -47 percent for IJSA+

Therefore, none of the treatments in Florida were cost-effective for government as a whole.

d. **Society**

Since none of the treatments were cost-effective from the perspective of either claimants or government, none were cost-effective for society as a whole. All three treatments in Florida imposed net costs to society: $1,004 per SJSA participant, $215 per IJSA participant, and $61 per IJSA+ participant. The net costs correspond to the following rates of return:

- -416 percent for SJSA
- -222 percent for IJSA
- -59 percent for IJSA+

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[12](...continued)
magnitude of the error in measuring the costs of the demonstration would need to be unrealistically large to change our conclusion that the more expensive structured treatment imposed net costs on DOL.

These results indicate clearly that none of the treatments were cost-effective. The individualized treatments were less expensive with respect to both demonstration costs and reduced earnings, and therefore were less costly to society.

3. Interpreting the Estimates of Cost-Effectiveness in D.C. and Florida

The evidence on the benefits and costs of the JSA treatments supports the following three conclusions:

- From the perspective of DOL-sponsored programs, none of the treatments in either state were cost-effective.
- From the perspectives of government defined more broadly and society (including UI claimants), all three treatments in D.C. and none of the treatments in Florida were cost-effective.
- It is unclear whether one treatment was more cost-effective than the others.

The three conclusions listed above are supported by the estimated rates of return reported in Table X.1 and Table X.2. Our confidence in these conclusions is bolstered by the degree of precision with we were able to estimate the rates of return. Our rate-of-return estimates depend heavily on (1) the estimates of the earnings impacts reported in Chapter VII and (2) the estimates of reductions in UI payments reported in Chapter V. Therefore, the precision of our rate-of-return estimates depend on the precision with which earnings impacts and reductions in UI payments were measured. In the remainder of the chapter, we address the precision of our estimates and the degree of confidence we have in our three conclusions.

None of the treatments in either state were cost-effective for DOL. The estimated rates of return to DOL rely on the estimated reductions in UI payments due to the JSA treatments. For IJSA+ in D.C. and SJSA in Florida, the estimated reductions are statistically different from (i.e. smaller than) the reductions that would be required for DOL to "break even" on those treatments. Therefore, we can

conclude with a high degree of confidence that those treatments were not cost-effective for DOL. However, while estimated rates of return from the other treatments are negative, the estimated reductions in UI payments are not statistically different from the reductions that would be required for DOL to "break even".[13] Therefore, it is possible that DOL broke even on the individualized treatments. However, it is very unlikely that DOL reaped even small net benefits from any of the treatments in either state.

All of the treatments in D.C. and none of the treatments in Florida were cost-effective for government as a whole and society. This conclusion rests on the estimated impacts on earnings and the additional tax revenue generated by those earnings. In D.C., the estimated earnings impacts were positive, leading to positive estimates of the rates of return to the three treatments. In Florida, the estimated earnings impacts tended to be negative or zero, leading to negative estimates of the rates of return. The strength of the evidence supporting the conclusion that JSA treatments were cost-effective in D.C. but not in Florida rests on the precision of the earnings impact estimates.

For SJSA in D.C., the estimated earnings impacts are positive and statistically significant for all quarters except the first. Therefore, we are fairly confident that SJSA was cost-effective in D.C. However, for the other treatments in D.C., the earnings impacts were not consistently significant. For both IJSA and IJSA+, the earnings impact estimates are both positive and statistically significant for three of the ten quarters. Therefore, in D.C., we are more confident in SJSA's cost-effectiveness than the cost-effectiveness of the individualized treatments.

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[13] In D.C., SJSA appears to be closest to "breaking even", with a rate of return equal to -44%. To be cost-effective, SJSA would have to have reduced UI payments in year 1 by $308 per claimant. We estimate that the impact was a reduction of $182 per claimant (Table V.1). Because our estimated impact of $182 is not significantly different from $308, we cannot rule out the possibility that DOL broke even on their investments in SJSA.

In Florida, although the estimated impacts on earnings tended to be negative, the actual impacts are likely to have been zero: the impact estimates were insignificant in all quarters for all three treatments. If the earnings impacts were zero in Florida, we would still conclude that none of the treatments reaped net benefits to government as a whole or society. However, the imprecision of the earnings impacts allows for the possibility that DOL broke even on the individualized treatments in Florida.

It is unclear whether one treatment was more cost-effective than the others. Because the estimated rate of return is higher for SJSA (151 percent) than for IJSA or IJSA+ (109 and 17 percent, respectively) in D.C., it is tempting to conclude that SJSA was more cost-effective in D.C. than the individualized treatments. However, this conclusion depends on whether the earnings impacts were larger for SJSA than for the individualized treatments. While our estimates of quarterly earnings impacts were largest for SJSA, the earnings impacts were not measured precisely enough to distinguish statistically between the impacts of the three treatments. Furthermore, in Florida, our estimates suggest that SJSA was even less cost-effective (i.e. imposed greater net costs) than the individualized treatments. Therefore, we conclude that the demonstration does not provide enough evidence to determine which treatment was most cost-effective.

In summary, the JSA treatments did not save money for DOL in either state. However, in D.C., they *did* produce net benefits to claimants--and were cost-effective from the perspectives of government as a whole and society. This conclusion is consistent with results from the New Jersey UI Reemployment Demonstration, which also yielded net benefits to UI claimants and government. The estimated earnings impacts and net benefits were even greater in D.C. than in New Jersey. In contrast, the JSA treatments in Florida failed to generate additional earnings and net benefits for claimants, and were not cost-effective from the perspective of government or society.

XI. CONCLUSION

The Job Search Assistance Demonstration tested different models for providing extensive job search services to UI claimants early in their UI spells. The models varied in the degree to which services were matched to individual needs. One treatment, SJSA, required each claimant to participate in the same set of services, while the other two treatments, IJSA and IJSA+, attempted to customize the services. In each treatment, claimants who did not participate in the required services were at risk of being denied part or all of their UI benefits.

In this chapter, we summarize our findings on the implementation and impacts of each treatment. Our discussion highlights important differences we found between the treatments. For example, we found that claimants assigned to IJSA and IJSA+ tended to participate in fewer group JSA services than we expected when the demonstration was designed. Hence, the differences in service receipt between the SJSA claimants and the IJSA and IJSA+ claimants were substantial. The differences in impacts between the treatments are less clear. In D.C., the SJSA treatment reduced UI receipt by significantly more than the IJSA and IJSA+ treatments. In Florida, however, the impact of the SJSA treatment on UI receipt was similar to the impacts of the IJSA and IJSA+.

Our final objective in this chapter is to use our findings as the basis for some conclusions about the Worker Profiling and Reemployment Service (WPRS) systems that states have recently implemented. Since WPRS uses the same method to target services to claimants as the demonstration, and the same method to provide services as was used in the IJSA treatment, the demonstration findings are very relevant for WPRS systems. We also compare our findings with findings from the national evaluation of the WPRS systems.

A. IMPLEMENTATION OF THE JSA DEMONSTRATION

The demonstration was, for the most part, successfully implemented in both D.C. and Florida. The first step in implementing the demonstration was to target services to an appropriate set of claimants. Both states were successful in using the two-stage claimant selection process to target demonstration services to claimants likely to face long UI spells. Claimants who were eligible for the demonstration but denied services (the control group) had longer average UI spells and were more likely to exhaust their benefits than claimants who were ineligible for the demonstration. In Florida, for example, the benefit exhaustion rate was about 6 percentage points higher for the demonstration-eligible claimants in the control group than for the ineligible claimants.[1] In D.C., the exhaustion rate was about 13 percentage points higher for the eligible claimants. The differences in average UI spells between eligible and ineligible claimants was 2 weeks in Florida and 1.5 weeks in D.C. These differences are not huge, but they are probably what would be expected from a statistical model of the determinants of benefit exhaustion among individual UI claimants.

Both states generally offered the services as they were designed for each of the three demonstration treatments. Claimants assigned to SJSA were offered the full set of mandatory treatment services, including orientation, testing, job search workshop, and assessment. The majority of claimants assigned to the demonstration attended at least the orientation, and the majority of those attending the orientation also attended testing, the workshop, and assessment. Most of the claimants who failed to attend any services did so because they were re-employed and/or had stopped collecting UI benefits.

Claimants assigned to the IJSA and IJSA+ treatments were also offered the full set of services, but few of these claimants participated in the JSA group services beyond orientation and assessment. Orientation and assessment were the only services that were mandatory for all ISJA and ISJA+ claimants

who continued to collect benefits. These claimants were required to participate in the other group services--testing and the workshop--only if these services were part of the service plan created in their assessment interview. Few claimants in either state participated in testing or the workshop. Attendance was especially low in D.C., where less than 1 percent of UISA and UISA+ claimants who attended orientation also attended testing or the workshop. Analogous attendance rates in Florida were higher but still modest--in the 10 to 20 percent range.

The low attendance rates in testing and the workshop suggest that demonstration claimants were reluctant to participate in services that were not mandatory. Furthermore, although UISA and UISA+ claimants were offered testing and the workshop, JSA staff were reluctant to make these services mandatory. Presumably, caseworkers either felt the services were inappropriate for most claimants or did not want to jeopardize claimants' benefits by making participation mandatory. Given this, the services received by SJSA claimants, who were automatically required to participate in the group services, substantially exceed those received by the UISA and UISA+ claimants.

To generate substantial rates of participation in group services, an ongoing program would probably need to make these services mandatory, as was done in SJSA. Claimants are reluctant to volunteer for services, as has been shown in this and other demonstration evaluations.[1] Staff are unlikely to aggressively assign claimants to services. Given the reluctance of both claimants and staff to initiate service participation, the only way to ensure participation is to make benefits contingent on service participation.

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[1]In the Pennsylvania Re-employment Bonus Demonstration, 3 percent of claimants who were offered a job search workshop on a voluntary basis actually participated in at least one session. Researchers concluded that the main reason for the low participation rate was the general lack of interest (Corson et al. 1992).

D.C. appears to have placed greater emphasis on individual counseling than on the group services for IJSA and IJSA+ claimants. Nearly half of the claimants assigned to the IJSA or IJSA+ groups participated in counseling. As explained in Chapter IV, the emphasis on individual counseling in D.C. may have arisen because of the limited space and trained staff to conduct group services in the D.C. office.

The timing of the JSA services was consistent with the demonstration design, which was based on the key objective of achieving early intervention. During the design phase of the demonstration, it was determined, given the time needed to identify and notify claimants, that services would ideally begin about 7 weeks after the initial UI claim. Our findings on the timing of participation show that the demonstration generally achieved early intervention according to this standard. Average time from the beginning of the benefit year to orientation was about 7 weeks in both states, and about 80 to 85 percent of claimants participated in orientation within 8 weeks of the beginning of the benefit year. Most claimants also moved on quickly to subsequent services. Claimants assigned to SJSA typically finished all services by the end of the second full week after orientation. Claimants assigned to IJSA or IJSA+ usually completed assessment within a week of orientation.

Data show that few demonstration claimants, even those assigned to IJSA+, participated in training under the Economic Dislocation and Worker Adjustment Assistance (EDWAA) program. However, the training rate was higher among the combined treatment groups than the control group. In Florida, the training rate was 3.5 percent for the combined treatment groups compared with 2.8 for the control group. In D.C., the corresponding training rates were 1.3 percent and 0.8 percent. These numbers imply that the information provided through orientation and assessment--the services that were mandatory in all treatments--was effective in inducing claimants to participate in EDWAA training. The effect, however, was fairly small and the resulting rate of training participation was also small.

Since the training rate was no higher among the IJSA+ groups than among the other treatment groups, we conclude that the IJSA+ approach was ineffective in providing greater access to training. Its ineffectiveness can be attributed to at least two reasons. First, demonstration claimants were not treated as automatically eligible for EDWAA, as was expected when the demonstration was designed. In most sites, claimants had several eligibility or procedural hurdles to clear before they could enter EDWAA training. This greatly slowed their potential entry into training. Second, coordination between the local demonstration sites and the local EDWAA programs often fell short of our expectations. EDWAA staff did not always participate in the demonstration services as they were designed, so in some sites, IJSA+ provided no better access to EDWAA.

Based on findings presented in previous JSA reports, we know that both states monitored and enforced the JSA participation requirements, but staff in the two states differed in their attitude and approach. Demonstration staff in both states told claimants that participation in the demonstration was mandatory and that claimants could lose their benefits if they refused to participate, but staff in Florida tended to downplay these aspects. Although staff in both states contacted claimants who failed to show for required services, D.C. tended to be more rigorous than the Florida sites, on average, in enforcing the requirements. In D.C., claimants who missed a single service were sent a noncompliance notice instead of their UI check, and they were required to report to the demonstration office to meet with a claims examiner and collect their benefit check. In contrast, most Florida offices allowed no-shows to maintain their benefits and reschedule missed services over the phone rather than in person. Benefit checks in Florida were held up only if claimants missed multiple appointments.

These findings demonstrate that states are likely to enforce similar participation requirements differently. States will come to different decisions about what constitutes noncompliance and how to warn claimants that they are in danger of losing their benefits.

B. IMPACTS OF THE JSA DEMONSTRATION

We estimated impacts of each of the demonstration treatments on various measures of UI receipt, benefit determinations and denials, employment and earnings, job characteristics, and job search activities. The treatments were expected to increase search efforts, speed re-employment, and reduce UI benefits.

For the most part, we found that the JSA treatments did tend to have modest impacts on these outcome measures. Some of the clearest impacts occurred for the UI measures. The JSA treatments each reduced UI receipt. The largest impact occurred for SJSA in D.C., which reduced UI receipt by more than a week. The other five JSA treatments across the two states had more modest impacts, reducing UI receipt by about half a week.[2] Most of the treatments also reduced the proportion of claimants who exhausted their benefits, with the estimated reduction ranging from about 2 to 5 percentage points. The impacts on dollars of UI receipt were somewhat smaller than would be expected given the impacts on UI weeks paid. The discrepancy arises because the impacts on weeks paid were larger for claimants with

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[2]In our estimates we have assumed that the treatments affected UI receipt only for claimants assigned to the treatment groups. This approach may overstate the UI savings generated by the treatments if the treated claimants, in their effort to hasten their own re-employment and exit UI, "displaced" other unemployed workers, who were forced to wait longer to become re-employed and exit UI. Whether such displacement occurred in the demonstration is unclear and is impossible to detect with the available data.

small weekly benefit amounts. With respect to timing of the impacts on UI receipt, we found that the treatment-control differences in UI exit rates occurred early in UI spells, around the time that claimants were notified of JSA service requirements or would have been scheduled to participate in services. This finding implies that much of the impact on UI receipt is due either to assignment to a treatment group or to the provision of services, rather than to a gradual application of the skills learned during the services.

None of the treatments had a significant impact on UI receipt beyond the initial benefit year. This finding is consistent with our expectations--we expected the treatments would help claimants become re-employed more quickly but have no effect on longer-term job stability. At the same time, our findings are inconsistent with those from the New Jersey UI Re-employment Demonstration, which showed that a structured JSA package in that state generated a significant reduction in UI receipt in the second year after the initial claim.

These findings provide no definitive conclusion about which service strategy is most effective in reducing UI spells. In D.C., SJSA generated a larger reduction in UI spells (more than one week) than IJSA and IJSA+ (about half a week), and the difference is statistically significant. But in Florida, the impact of SJSA was nearly identical to that of IJSA and IJSA+. Given these findings and our information about the enforcement policies in the two states, we conclude that the SJSA approach is likely to generate larger UI reductions only in settings where the additional participation requirements associated with SJSA are strictly enforced. Another factor possibly contributing to the large impact of SJSA in D.C. is that claimants there traditionally have had long spells on UI, so there was substantial potential for reduction in average UI spells in response to the demonstration treatments. However, our subgroup analysis, which revealed no evidence that impacts were larger for claimants with high predicted probabilities of exhaustion, tends to contract this possibility.

All of the JSA treatments increased benefit eligibility determinations and denials. When broken down by reason, most of the extra determinations and denials were related to regular UI benefit eligibility rather than directly to JSA participation. It appears that local staff used the information gathered through the demonstration to increase enforcement of traditional UI eligibility requirements. The increased benefit denials were probably responsible for part of the impact of the treatments on UI receipt, especially in D.C. where the impact of the treatments on benefit denials was largest.

The JSA treatments had somewhat uneven impacts on post-UI employment and earnings. On one hand, the SJSA group in DC generally had higher earnings than the control group, and the differences tended to be statistically significant. Furthermore, the quarterly earnings impacts were fairly large (approximately $200 per quarter) and persistent over the 2.5 year follow-up period: the earnings impact in the tenth quarter was $224 dollars. On the other hand, the estimated impacts of SJSA on quarterly earnings in Florida were typically negative though small and statistically insignificant, and the estimated impacts of IJSA and IJSA+ were modest in DC and often negative in Florida, although again the estimates were not statistically significant. The impacts on employment rates were similar to the impacts on earnings, though we find small positive impacts of IJSA on employment. Overall, we find positive impacts on earnings and employment for SJSA in DC, more modest impacts of the individualized treatments in DC, and no positive impacts for the JSA treatments in Florida.

We also examined the JSA impacts on UI receipt and employment for different subgroups of claimants, but our estimates lead us to few solid conclusions. The pattern of the estimated impacts on UI receipt differ between D.C. and Florida. In D.C., for example, the JSA impacts on UI tend to be particularly large for young claimants and white claimants. In Florida, the impacts on UI receipt tend to be large for women and those claimants previously employed in trade industries. In neither state do we find

any evidence that impacts were larger for claimants with high predicted probabilities of exhaustion. These results suggest that making the eligibility criteria more restricted by raising the exhaustion probability threshold would have no substantial effect on the UI and employment impacts.

We found no evidence that any of the JSA treatments pushed claimants into lower-quality jobs in order to hasten their re-employment. On the contrary, the treatments appear to have potentially improved the quality of the jobs participants accepted. The treatments also did not affect the likelihood that claimants switched occupations.

At least part of the impacts of the JSA treatments on UI receipt and earnings probably occurred because the treatments encouraged more aggressive job searches among treatment group members. In both D.C. and Florida, each of the JSA treatments led claimants to contact more employers per week as part of their job search. The IJSA and IJSA treatments also increased the likelihood that claimants in D.C. searched for employment and increased the hours that claimants in Florida spent searching for work.

Another potential impact of the demonstration was to increase claimants' use of the state Job Service (JS) to assist in their job search. All of the JSA treatments increased contact with the JS as designed, and they also tended to increase the probability that claimants received job referrals from the JS. However, there was no evidence of impacts on the likelihood of obtaining job offers through JS referrals. Thus, it appears that the JSA demonstration was successful in getting claimants to use the JS, but less successful in translating the use of these services into employment opportunities generated directly by the JS.

When we sum up the impacts on different outcomes and account for the costs of services, we find that the JSA treatments were not cost effective from the perspective of DOL. None of the treatments in either state generated a positive return on the resources invested by DOL--the estimated reductions in UI payments caused by the treatments were not large enough to fully compensate DOL for the costs of the

services. The best case scenario implied by our estimates is that DOL would break even on an investment in JSA.

Although the JSA treatments were not generally cost-effective from DOL's perspective, they may have been cost-effective from a broader perspective. The D.C. treatments generated substantial returns from the perspectives of government and society as a whole. For example, the societal return for the SJSA treatment was 826 percent, which implies that one dollar invested in SJSA yielded $8.26 in benefits for society. In contrast, the same treatments in Florida failed to generate positive returns for either the government or society as a whole. We hesitate to draw strong conclusions on the returns to society based on the Florida findings, since these estimates are sensitive to the earnings impacts, which are imprecisely estimated. The negative returns in Florida are driven partly by the finding that the treatments negatively affected earnings, but these negative earnings estimates are statistically indistinguishable from zero.

The net benefit findings for D.C. are similar to those found in the New Jersey UI Re-employment Demonstration, while those for Florida are less favorable. In both D.C. and New Jersey, structured JSA packages generated net benefits for claimants, society, and the government as a whole, largely because of earnings increases in both states. In contrast, the Florida net benefit estimates are less favorable because of the lack of earnings impacts generated by SJSA in that state.

C. IMPLICATIONS FOR WORKER PROFILING AND RE-EMPLOYMENT SERVICES

The Unemployment Compensation Amendments of 1993 required states to develop Worker Profiling and Re-employment Services (WPRS) systems to identify UI claimants who might benefit from re-employment services and then refer them to re-employment services. These amendments essentially directed all states to build their own statewide job search assistance systems. For WPRS, states are required to use the same two-step approach used in the demonstration to identify claimants to be referred

to services. In most states, service referral in WPRS is similar to the UISA treatment in the demonstration—each claimant is required to meet one-on-one with a counselor to develop an individual service plan and assess the claimant's interests and abilities (Dickinson et al. 1999). Most states in WPRS also require at least some claimants to participate in individualized services beyond the standard mandatory services. However, as in UISA, the percentage of claimants in any state actually required to participate in additional individualized WPRS services may be fairly low.

The demonstration findings suggest that the typical WPRS service approach, which does not automatically require claimants to participate in services beyond orientation and assessment, is unlikely to generate widespread participation in other group services such as testing or job search workshops. To generate widespread participation, the states probably need to mandate these services. Findings from the WPRS evaluation presented in Dickinson et al. (1999) are largely consistent with this argument. Among the five states with valid data on service participation, the two states that explicitly required claimants to participate in a job search workshop as part of their WPRS requirements (New Jersey and Maine) generated fairly high workshop participation rates—about 40 percent or more. The other three states (Connecticut, Illinois, and South Carolina), which did not have explicit workshop requirements, generated much lower participation rates. Hence, it appears that in the early days of WPRS, substantial participation in many services was only achieved through explicit requirements that were backed up by the threat of benefit denials.

Recommendation: If states want to expand services received by claimants through WPRS, states should make particular services mandatory for all claimants referred to WPRS, or at least encourage local offices to be aggressive in using individual service plans to set and enforce service requirements.

Findings from the demonstration also suggest that coordination under WPRS between UI/JS and local agencies authorized to provide training under the Workforce Investment Act (WIA) may be difficult. In both of the JSA demonstration states, as explained above, demonstration staff had some difficulty in working with EDWAA staff and getting claimants into EDWAA training quickly. This is consistent with early observations of the WPRS systems presented in Hawkins et al. (1995), which reports that in many of the subject states, EDWAA played little or no role in WPRS. The researchers argue that improved linkages between EDWAA and the local UI and JS agencies involved in WPRS would allow the agencies to take better advantage of EDWAA expertise in serving dislocated workers with diverse needs. Coordination between UI/JS and EDWAA may have improved over time. Based on responses to a 1997 survey, Dickinson et al. (1999) report that in 50 percent of states, EDWAA was substantially involved in at least one major WPRS task. Furthermore, EDWAA has now been replaced by WIA. The WIA requirement that local areas establish One-Stop Career Centers, which bring multiple agencies together in a single location to serve all clients, should contribute to improved coordination between UI/JS and the WIA agencies.

> **Recommendation:** DOL should continue to develop new tools, in addition to the One-Stop Career Centers, to encourage coordination of UI/JS and WIA and increase the exposure of WPRS claimants to WIA services.

WPRS participation requirements are likely to increase UI nonmonetary benefit determinations and denials. Some of the increase will be due to direct enforcement of the WPRS requirements. But much of the increase will be due to more strict enforcement of traditional UI eligibility requirements. This kind of enforcement will be possible because of the additional information that local offices collect from claimants

to track WPRS activities. Dickinson et al. (1999) confirm that WPRS increased nonmonetary benefit determinations and denials in most of the states that they examined.

The JSA demonstration findings suggest that WPRS generates modest reductions in UI receipt. According to our estimates, the IJSA treatments, which most resembled typical WPRS services, reduced UI receipt by about half a week. Estimates from the WPRS evaluation reported in Dickinson et al. (1999) confirm that WPRS has an impact on UI receipt. WPRS reduced UI receipt in four of the six states investigated by Dickinson et al., with estimated reductions in the four states ranging from one-quarter of a week to one full week of benefits.

Implications of the JSA demonstration findings for the impacts of WPRS on employment and earnings are more mixed. The IJSA treatments increased earnings in some quarters in D.C., but we found no clear evidence that the treatments increased earnings at all in Florida. Dickinson et al. also found no clear evidence that similar services in WPRS increased employment or earnings, even in the states where UI receipt was significantly reduced.

Finally, our findings provide little evidence that moving WPRS to a more structured model would be cost-effective. While in D.C. the rate of return on investment in SJSA was somewhat higher than on investment in IJSA, in Florida we found just the opposite. Furthermore, these comparisons are very sensitive to the earnings impacts, which are estimated imprecisely.

Recommendation: Structured services do not necessarily maximize cost-effectiveness. States should use structured services only if their primary objective in WPRS is to expand service participation.

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APPENDIX A

SURVEY RESULTS AND NONRESPONSE BIAS

The JSA follow-up survey was designed to collect data primarily related to the post-claim experiences of claimants. Interviews with treatment and control group members were conducted by telephone approximately one year after their initial claim. Interviewing began in March 1996 and continued until August 1997. The interviews included questions about service participation, job search activities, pre-UI employment and earnings, post-UI employment and earnings, and personal characteristics. This appendix presents the results of this survey and examines potential nonresponse bias that could arise with use of these data.

A. SURVEY RESULTS

This section summarizes the results of the survey and discusses the major reasons for nonresponse. A total of 3,285 claimants were interviewed, with the totals distributed across treatment and control groups as shown in Table A.1. The relatively low number of respondents for the IJSA and IJSA+ groups reflects the design of the survey. Since there was little distinction in the field between the IJSA and IJSA+ treatments, the survey was designed so that the IJSA and IJSA+ groups were interviewed at half the rate of the SJSA and control groups. The intention was to combine the IJSA and IJSA+ respondents into a single group comparable in size to the SJSA or control group respondents. The statistics shown in Table A.1 suggest that this goal was achieved.

Table A.2 shows the final disposition of all survey contacts. Approximately 60 percent of the survey contacts were completed. The most common reason for an interview not being completed was not being able to contact an individual. This difficulty in reaching a large number of people probably reflects the increasing diversity of American households, where fewer people keep regular hours. None of the other reasons for uncompleted interviews account for a substantial amount. Only 5 percent of the sample refused to respond.

TABLE A.1

JSA FOLLOW-UP SURVEY SAMPLE SIZES

Treatment Group	District of Columbia	Florida
SJSA	425	703
IJSA	228	355
IJSA+	194	321
Control Group	419	640
Total	1,266	2,019

TABLE A.2

FINAL DISPOSITION OF INTERVIEWS

Final Status of Interview	Number	Percent
Completed	3,285	59.7
Not Completed	2,216	40.3
Deceased	28	0.5
Partial Complete	58	1.1
Refusal	279	5.1
Language Barrier	21	0.4
Illness or Disability	31	0.6
Could Not Locate	239	4.3
Located But Could Not Contact	1,339	24.3
Other	221	4
Total	5,501	100

SOURCE: JSA demonstration follow-up survey monitoring system.

Table A.3 illustrates the variation in attendance at JSA activities in D.C. for respondents and the nonrespondents in the treatment groups. There were no statistically significant differences between the respondents and the nonrespondents in attending any of the JSA activities. In Florida, however, the nonrespondents were generally less likely to participate in JSA activities, according to Table A.4. The SJSA respondents were significantly more likely to attend the assessment, the workshop, and the orientation than the SJSA nonrespondents. The IJSA respondents were also significantly more likely to attend the JSA assessment, the JSA workshop, and to complete the orientation than the IJSA nonrespondents. Among the IJSA+ group, the JSA workshop was the only activity the respondents were significantly more likely to attend. But even for the other activities, where there was no statistically significant difference, the direction of the difference between the IJSA+ respondents and nonrespondents suggests the respondents were more likely to attend the JSA activities.

The pattern of differences between respondents and nonrespondents that was observed in Florida suggests that nonresponse was linked to not participating in the demonstration altogether. Perhaps many of the nonrespondents in Florida felt the entire JSA demonstration was too burdensome and declined to participate in the survey and the JSA services. Employment opportunities were more abundant in Florida, and consequently many claimants may have felt they could secure employment easily without having to participate in the JSA demonstration. In D.C., however, there is less evidence of this.

B. POTENTIAL NONRESPONSE BIAS

The survey-based impact estimates presented in this report could be biased if the nonrespondents were systematically different from the respondents. To the extent that

TABLE A.3

JSA SERVICE ATTENDANCE BY D.C. SURVEY RESPONSE STATUS
(Percent)

	SJSA		IJSA		IJSA +	
	Respondents	Non-respondents	Respondents	Non-respondents	Respondents	Non-respondents
Attended Orientation	59.2	55.0	62.4	60.5	55.8	58.4
Attended Job Search Workshop	56.1	52.2	2.6	2.6	1.4	2.9
Attended Testing	59.0	54.7	1.1	2.1	1.4	3.5
Attended Assessment	64.3	64.2	76.7	75.2	75.6	71.7

SOURCE: JSA demonstration participant tracking system.

TABLE A.4

JSA SERVICE ATTENDANCE BY FLORIDA SURVEY RESPONSE STATUS
(Percent)

	SJSA		IJSA		IJSA +	
	Respondents	Non-respondents	Respondents	Non-respondents	Respondents	Non-respondents
Completed Orientation	62.7	55.8**	68.1	59.7*	63.8	6.1
Attended Job Search Workshop	46.7	38.2***	10.6	4.7***	10.5	5.0*
Attended Testing	42.6	38.2	12.5	8.3	8,4	6.8
Attended Assessment	47.0	39.2**	64.2	57.1*	60.4	56.41

SOURCE: JSA demonstration participant tracking system.

*Difference between respondents and nonrespondents is statistically significant at 90 percent level of confidence in a two-tailed test.
**Difference between respondents and nonrespondents is statistically significant at 95 percent level of confidence in a two-tailed test.
***Difference between respondents and nonrespondents is statistically significant at 99 percent level of confidence in a two-tailed test.

nonrespondents differed substantially from the respondents, our impact estimates would only be valid for that proportion of the demonstration population that are represented by the respondents and not the entire demonstration population. In this section, we explore the likelihood that nonresponse may have biased our survey-based impact estimates.

To determine the potential for nonresponse bias, we used administrative data that are available for both respondents and nonrespondents. These data include both demographic and other baseline data and data on UI benefits, earnings, and employment. By using administrative data to gauge the extent of nonresponse bias, we are implicitly assuming that differences between respondents and nonrespondents in administrative data closely parallel differences that would have existed between these two groups in the survey data.

Table A.5 illustrates differences between nonrespondents and respondents in D.C. on demographic characteristics and pre-UI wages. The nonrespondents were more likely to be male, tended to be younger, had lower pre-UI earnings, and were entitled to less UI benefits than the respondents. Differences between respondents and nonrespondents in UI receipt, employment, and earnings were less common and appear to be concentrated among the SJSA claimants, as Table A.5 shows. The SJSA nonrespondents were more likely to exhaust their UI benefits, typically had lower earnings, and were less likely to be employed in two of the three quarters.

Because our impact estimates are based on differences between the treatments and the controls, it is necessary to consider if differences between the control respondents and nonrespondents paralleled the differences between the SJSA respondents and nonrespondents. The last columns in Table A.5 suggest they did not, and the fact that the control nonrespondents do not exhibit such a pattern indicates the survey-based impact estimates for SJSA claimants in D.C. may be somewhat biased estimates of the impacts for the full sample. Had the differences between the control

TABLE A.5

CHARACTERISTICS OF D.C. SURVEY SAMPLE BY TREATMENT GROUP AND SURVEY RESPONSE STATUS

	Structured JSA		Individualized JSA		Individualized JSA +		Control Group	
	Respondents	Nonrespondents	Respondents	Nonrespondents	Respondents	Nonrespondents	Respondents	Nonrespondents
Male	39.5%	49.4%***	43.8%	48.6%	38.7%	48.3%*	40.8%	47.9%**
Black	71.5%	75.0%	75.4%	71.1%	71.6%	72.9%	71.8%	76.6%
Latino	4.2%	4.3%	3.1%	2.9%	6.7%	3.9%	3.8%	4.2%
Other	1.8%	1.8%	1.0%	1.1%	1.0%	1.3%	1.7%	1.2%
Age	40.2	35.7***	39.4	37.5*	38.4	35.6***	38.9	37.2**
Base Period Earnings	$19,624	$17,040***	$19,394	$16,884**	$19,044	$17,926	$18,982	$17,255
UI Benefits								
Entitlement	$5,679	$5,256***	$5,512	$5,110**	$5,443	$5,348	$4,837	$5,008
Amount Paid	$4,064	$4,001	$4,412	$4,094	$4,228	$4,214	$4,327	$4,098
Weeks Paid	18.2	19.2	19.8	19.7	19.3	20.0	20.5	19.8
Exhaust Rate	48.2%	56.2%**	53.1%	54.1%	54.1%	60.6%	59.4%	60.0%
Earnings[a]								
Quarter 1	$1,607	$1,147	$1,213	$1,358	$1,106	$877	$1,236	$1,085
Quarter 2	$2,094	$1,545**	$1,565	$1,575	$1,863	$1,108***	$1,700	$1,389*
Quarter 3	$2,265	$1,602***	$2,036	$1,955	$2,152	$1,483**	$1,870	$1,704

SOURCE: State UI claims records; state UI wage records.

[a]Quarters 1,2, and 3 are the first, second, and third full calendar quarters following initial UI claim.

*Statistically significant at the 90 percent confidence level in a two-tailed test.

respondents and nonrespondents mirrored the differences between the SJSA respondents and nonrespondents, then nonresponse bias would be less of a concern.

In contrast, there do not appear to have been substantial systematic differences between the respondents and the nonrespondents among the IJSA and IJSA+ claimants. Therefore our survey based IJSA and IJSA+ impact estimates in D.C. are unlikely to be biased.

Table A.6 illustrates differences between survey respondents and nonrespondents on demographic characteristics and UI benefits and earnings in Florida. As was the case in D.C., nonrespondents were more likely to be male, tended to be younger, earned significantly less, and were entitled to less UI benefits than respondents in Florida. These patterns hold across the different treatment groups.

Table A.6 suggests the differences in Florida between respondents and nonrespondents on outcome measures are most stark among SJSA and the IJSA claimants. The SJSA nonrespondents in Florida collected fewer UI benefits for a shorter period of time and also had significantly less earnings in all three post-UI quarters. The IJSA nonrespondents exhibit a similar pattern where they collected fewer UI benefits for a shorter period of time, and they were less likely to exhaust their benefits. The IJSA nonrespondents also have significantly lower earnings in quarters 1 and 3. Although the differences between the respondents and nonrespondents were not as stark among the IJSA+ and control claimants, the overall pattern was similar to what was observed for SJSA and IJSA claimants. The IJSA+ and control nonrespondents tended to collect fewer UI benefits over a shorter period of time and their earnings tended to be lower. Even when the differences between the respondents and nonrespondents are not statistically significant, they are not trivial.

TABLE A.6

CHARACTERISTICS OF FLORIDA SURVEY SAMPLE BY TREATMENT GROUP AND SURVEY RESPONSE STATUS

	Structured JSA		Individualized JSA		Individualized JSA+		Control Group	
	Respondents	Non-respondents	Respondents	Non-respondents	Respondents	Non-respondents	Respondents	Non-respondents
Male	50.0%	60.3%***	49.0%	58.9%**	60.0%	59.4%	51.9%	56.9%*
Black	13.6%	17.8%*	14.9%	18.9%	15.2%	17.1%	16.1%	18.2%
Latino	20.6%	22.4%	21.4%	29.4%***	18.1%	26.7%**	21.5%	21.4%
Other	1.8%	1.0%	2.2%	2.6%	1.6%	1.8%	1.6%	1.1%
Age	44.4	40.4***	45.9	41.6***	42.8	41.4	44.3	40.9***
Base Period Earnings	$20,310	$17,210***	$19,200	$16,900*	$20,140	$16,490***	$20,270	$17,560***
UI Benefits								
UI Entitlement	$4,258	$3,782***	$4,029	$3,705*	$4,288	$3,771***	$3,872	$3,244***
Amount Paid	$2,770	$2,428***	$2,695	$2,236***	$2,744	$2,380**	$2,888	$2,574***
Weeks paid	15.68	14.37**	15.44	13.55**	15.30	14.54	16.64	15.38**
Exhaust Rate	42.6%	40.7%	45.9%	36.3%**	41.1%	40.1%	44.7%	44.5%
Earnings[a]								
Quarter 1	$1,976	$1,317***	$2,084	$1,464**	$2,023	$1,486	$1,687	$1,329**
Quarter 2	$2,291	$1,981*	$2,499	$2,132	$2,577	$1,973**	$2,176	$1,890*
Quarter 3	$2,744	$2,238***	$2,900	$2,344*	$2,811	$2,472	$2,574	$2,236*

SOURCE: State UI claims records; state UI wage records.

*Statistically significant at the 90 percent confidence level in a two-tailed test.
**Statistically significant at the 95 percent confidence level in a two-tailed test.
***Statistically significant at the 99 percent confidence level in a two-tailed test.

[a]Quarters 1,2, and 3 are the first, second, and third calendar quarters following initial UI claim.

Although there appear to be substantial differences in Florida between the three treatment group nonrespondents and the respondents on many of the outcome measures, this does not necessarily mean our impact estimates are biased. If the differences between the control respondents and nonrespondents parallel those found among the treatments, then our impacts estimates are unlikely to be seriously affected by nonresponse. The figures presented in the last two columns of Table A.6 suggest that this might be the case. Among the Florida controls, nonrespondents received less UI benefits over a shorter period of time and earned significantly less in one of the three quarters. The size and pattern of differences are generally consistent with what we observed among the Florida treatment groups. This suggests that survey data may not provide accurate data on mean outcomes for the full demonstration sample, but the impact estimates based on the survey data would not appear to be substantially biased.

To bring into sharper focus the degree to which nonresponse may have biased our estimates, we compared our estimates of JSA impacts on UI receipt and earnings, which were based on the entire demonstration sample (Chapters V and VII), with corresponding impact estimates based on a sample that includes only the survey respondents. This simulation assumes that our survey-based estimates parallel the full sample. Tables A.7 and A.8 illustrate the impacts for the full sample and the respondents for D.C. and Florida, respectively.

The findings for both states provide scattered evidence of nonresponse bias in the respondent-based estimates that overstates some of the impacts on the full sample. For example, in D.C. (Table A.7) the impacts on UI weeks and exhaustion for the SJSA survey respondents are more than twice the magnitude of the estimated impacts for the full sample. The differences tend to be smaller for the other outcomes and other treatments, and in some cases the differences are even in the opposite

TABLE A.7

DIFFERENCES BETWEEN FULL SAMPLE IMPACT AND IMPACTS INCLUDING ONLY RESPONDENTS
(D.C.)

	SJSA		IJSA		IJSA +	
	Full Sample	Survey Respondents	Full Sample	Survey Respondents	Full Sample	Survey Respondents
Weeks of UI Receipt in Benefit Year	-1.13***	-2.29***	-0.47**	-0.65	-0.61***	-1.2*
Dollars of UI Benefit in Benefit Year	-182**	-262*	-56	85	-37	-99
Exhaustion Rate (Percent)	-4.8***	-11.2***	-2.4*	6.4*	-3.9***	-5.3
Earnings[a] (Dollars)						
Quarter 1	30	371	21	-23	20	-129
Quarter 2	172**	395**	100	-135	145**	164
Quarter 3	215***	395**	126*	66	184**	282

SOURCE: State UI claims records; state UI wage records.

NOTE: The impact estimates are based on differences between the control and the JSA groups.

[a]Quarters 1,2, and 3 are the first, second, and third calendar quarters following initial UI claim.

*Statistically significant at the 90 percent level of confidence in a one-tailed test.
**Statistically significant at the 95 percent level of confidence in a one-tailed test.
***Statistically significant at the 99 percent level of confidence in a one-tailed test.

TABLE A.8

DIFFERENCES BETWEEN FULL SAMPLE IMPACT AND IMPACTS EXCLUDING NONRESPONDENTS
(Florida)

	SJSA		IJSA		IJSA +	
	Full Sample	Survey Respondents	Full Sample	Survey Respondents	Full Sample	Survey Respondents
Weeks of UI Receipt in Benefit Year	-0.41**	-0.96**	-0.59***	-1.20**	-.52**	-1.34**
Dollars of UI Benefit in Benefit Year	-39	-118	-100**	-193*	-73*	-144
Exhaustion Rate (Percent)	-1.8*	-2.0	-2.4**	1.2	-2.8**	-3.6
Earnings[a] (Dollars)						
Quarter 1	52	289*	-48	397*	-24	336
Quarter 2	-5	115	-6	323*	20	401**
Quarter 3	-54	170	-17	325*	14	237

SOURCE: State UI claims records; state UI wage records.

NOTE: The impact estimates are based on differences between the control and the JSA groups.

[a]Quarters 1,2, and 3 are the first, second, and third calendar quarters following initial UI claim.

*Statistically significant at the 90 percent level of confidence in a one-tailed test.
**Statistically significant at the 95 percent level of confidence in a one-tailed test.
***Statistically significant at the 99 percent level of confidence in a one-tailed test.

direction--the impacts on UI receipt or earnings are smaller for the respondents than for the full sample.

In Florida (Table A.8) the impacts on some UI outcomes and quarterly earnings tended to be larger in magnitude for the survey respondents than for the full sample. For example, the estimated impact of each treatment on weeks of UI receipt among on respondents was about twice the magnitude of the estimated impact for the full sample. The estimated quarterly earnings impacts were also consistently larger for the respondents than for the full sample. One outcome that was not consistent with this pattern was UI exhaustion--the impact of each treatment on exhaustion was similar for the two samples.

These findings suggest that we need to be aware that our survey-based estimates may suffer from some nonresponse bias that tends to overstate the treatment impacts. However, given the inconsistency of the apparent bias in our simulations, especially in D.C., we cannot know that all or even most of the survey-based estimates suffer substantially from nonresponse bias. Given this, and given that the survey is our only source of data for some outcomes, we have chosen to present several estimates based on the survey data in Chapters VIII and IX. However, our interpretations of the findings acknowledge that the estimates based on these data may be overstated to some extent.

APPENDIX B

VALIDITY OF THE JSA ELIGIBILITY CRITERIA

The JSA demonstration was designed to target job search assistance to UI claimants likely to exhaust their benefits in order to expedite re-employment. A critical component of the demonstration design was to effectively identify these particular claimants so that services could be targeted to them. In this appendix, we discuss the validity of the eligibility criteria in the JSA demonstration to determine whether these claimants were successfully identified.

Overall, the two-step claimant selection process used in the demonstration was successful in identifying the target group. Claimants who met the eligibility criteria were more likely to exhaust their UI benefits and had longer spells on UI than either claimants who did not pass the initial screens or claimants who passed the initial screens but did not meet the threshold for predicted probability of benefit exhaustion. Applying both the initial screens and the probability threshold screened out groups with relatively low probabilities of benefit exhaustion. Among the initial screens, the partial first-payment criterion was especially successful in identifying claimants in both Florida and D.C who were unlikely to have long UI spells, and the transitional claim and union hiring hall attachment criteria were also important in D.C.

In both Florida and D.C., claimants with high predicted probabilities of exhaustion were more likely to exhaust their UI benefits and to receive more UI benefits than claimants with low probabilities of exhaustion. A claimant's education and the local unemployment rate at the time a claimant filed for UI benefits were important predictors of whether the claimant experienced a long spell on UI in both states. Findings from both states also revealed that claimants from finance, insurance, and real estate industries were likely to have long spells and exhaust their benefits, while claimants previously employed in service occupations tended to have shorter spells.

In assessing the validity of the profiling models, we also examined whether the use of more recent data led to substantially different parameter estimates for the profiling model, and whether

these updated profiling models identified substantially different types of claimants as likely to exhaust their benefits. We updated the profiling models by re-estimating them using data obtained as part of the demonstration on claimants who passed the initial screens but were not assigned to a treatment. The results for the updated profiling models show that the parameter estimates based on the more recent data differ considerably from the parameters of the assignment model. In both states, the size of the effects of the unemployment rate and education changed substantially between the two time periods, and the ordering of effects within industry and occupation also changed considerably.[1] In Florida, the effects of tenure also changed dramatically between the two time periods.

Using more recent data to update the profiling models changed the group of claimants who would have been served, but it did not improve their ability to target claimants who were most likely to exhaust their benefits or to receive benefits for a long time. Based on quartiles of the predicted probabilities of exhaustion, 29 percent of claimants in D.C. placed in the top quartile by the assignment model were not placed in the top quartile by the updated profiling model. For Florida, the corresponding figure is 36 percent. These results show that updating the profiling model could dramatically alter which claimants received services. Furthermore, there were important differences in the characteristics of the claimants placed in the upper quartile of predicted exhaustion probabilities by the assignment and updated models. In contrast, differences in UI and employment outcomes were generally small and do not consistently indicate that the profiling models updated with more recent data are any more successful than the assignment model that was based on historical data. In D.C., claimants selected on the basis of the updated profiling model received

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[1]The data used to construct the original profiling models were from May 1993 to May 1995. The data used to update the models are from June 1995 to June 1996 in D.C. and March 1993 to March 1996 in Florida.

slightly more UI benefits and had somewhat lower earnings than claimants selected on the basis of the assignment model. They were also, however, less likely to exhaust their benefits. In Florida, claimants selected by the updated model were more likely to exhaust their benefits and have lower earnings, but their average benefit receipt was similar to that of claimants selected by the assignment model. These results suggest that the profiling model based on the more recent data was not unequivocally superior to the assignment model that was based on historical data. These findings further suggest that, for these states during this period, frequent updating of the profiling model with more recent data was not necessary.

The discussion that follows explores these results in greater detail. We begin with a description of the eligibility criteria used in the JSA demonstration. We then discuss results concerning the validity of the eligibility criteria as a whole and individually. Finally, we discuss the effects of updating the profiling models on their validity in identifying UI claimants likely to exhaust their benefits.

A. JSA DEMONSTRATION ELIGIBILITY CRITERIA

Both demonstrations used a two-step selection process. In the first step, characteristic screens identified a broad group of claimants of potential interest to the demonstrations who recently separated permanently from their employer. In the second step, the probability of benefit exhaustion was estimated for each claimant. Those with the highest predicted probabilities of exhaustion were targeted to participate in the demonstration. This approach follows the DOL prototype for targeting claimants for services in the new Worker Profiling and Re-employment Services system. The claimants targeted for the JSA demonstration based on their predicted probability of exhaustion were then randomly assigned to the treatment and control groups.

1. Initial Exclusion Screens

The first step of the two-step selection process employed a set of exclusion screens to identify a broad group of claimants of potential interest to the demonstration. Florida and D.C. used generally similar sets of exclusion screens in the first step. These screens were used to target claimants who were not attached to a job, and who had entered the UI system recently. Although some of the screens were expected to be associated with long UI spells, this was not the primary reason for their selection. Eight exclusion screens were used:

- ***Interstate Claims.*** In Florida, all interstate claims were excluded from the demonstration. In D.C., interstate claims were generally excluded from the demonstration, except those who filed in seven Maryland and Virginia offices just outside D.C.

- ***Transitional Claims.*** Both states excluded claimants receiving a first payment under a transitional claim (a claim that immediately follows the end of a benefit year).

- ***Labor Dispute.*** Both states excluded claimants involved in a labor dispute.

- ***Union Hiring Hall.*** Both states excluded claimants who obtain jobs through a union hiring hall.

- ***Temporary Layoff.*** Both states excluded claimants who had a specific date of recall to their previous employer. Because of its large agricultural sector, Florida also excluded claimants on a seasonal layoff.

- ***Partial Payment/Earnings.*** Both states excluded claimants who received a partial first payment because they had earnings during the week the first payment was made. It was assumed that claimants with earnings were job attached and did not need job search assistance.

- ***Late First Payment.*** Both states excluded claimants whose first payment date was 42 or more days after the benefit year beginning date.

- ***Distance.*** Florida excluded claimants who resided more than 21 miles from the office providing demonstration services.

In D.C., about 37 percent of claimants with a first payment were screened out using the initial eligibility criteria. The most important screens were those associated with delayed first payments, which accounted for 37 percent of screened-out claimants, and interstate claims, which accounted for 25 percent of screened-out claimants. Other important screens included partial first payments (18 percent) and union hiring hall (17 percent). Transitional claims accounted for 8 percent of screened-out claimants, and recall date accounted for about 7 percent. Only 0.2 percent were in labor disputes.[2]

In Florida, about 20 percent of claimants with first payments were screened out using the initial eligibility criteria. The most important screen in Florida was delayed first payment, which accounted for 27 percent of screened-out claimants. Other important screens include union hiring hall or seasonal worker status (19 percent of screened-out claimants), a recall date (18 percent), transitional claims (13 percent), and partial first payment (11 percent). About 5 percent were screened out because they lived more than 21 miles from the office providing JSA services, and another 7 percent were screened out but were missing data on their screens.

2. DOL Profiling Model

The second step of the claimant selection process entailed estimating a probability of exhaustion for each claimant that passed the first-stage screens. These models follow the DOL prototype for targeting claimants for services in the new Worker Profiling and Re-employment Services (WPRS) systems. The variables used in the profiling model are:

- ***Local Unemployment Rate.*** The unemployment rate in the geographic area where the claimant filed for UI benefits, measured in the quarter the claimant filed.

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[2]The percentages add to more than 100 percent because some claimants failed to pass more than one screen.

- ***Job Tenure.*** Years of employment with a claimant?s last employer.
- ***Education.*** Highest formal degree a claimant had at the time he or she filed for UI benefits.
- ***Occupation.*** Claimant's occupation in his or her last job.
- ***Industry***. Industry of claimant's last job.

Those claimants with the highest predicted probabilities of exhaustion were targeted by the PTS for random assignment to the demonstration. In Florida, claimants with an estimated exhaustion probability of 40 percent or greater were deemed eligible for the demonstration. In D.C., claimants with a predicted probability of 60 percent or greater were eligible. The probability threshold was set higher in D.C. than in Florida because, historically, rates of benefit exhaustion have been substantially higher in D.C. About 38 percent of claimants in D.C. who passed the initial eligibility screens were excluded from the demonstration because their predicted probability of exhaustion was below the threshold. In Florida, the comparable figure was 35 percent.

Given capacity constraints, demonstration sites typically served only a subset of claimants above the threshold. About 32 percent of those above the threshold in D.C. were not assigned to the demonstration, while in Florida 79 percent above the threshold were not assigned. Since those receiving services were chosen randomly from claimants above the threshold, services were directed to claimants with high probabilities of exhaustion.

B. VALIDITY OF ELIGIBILITY CRITERIA

A valid set of eligibility criteria should identify claimants likely to experience long spells of UI receipt and exhaust their benefits. In this section we present analyses of the effectiveness of the two sets of eligibility criteria in identifying such claimants. Some of the initial eligibility screens (i.e.,

distance from office providing JSA services, seasonal workers, and union hiring hall attachment) were not specifically designed to target claimants with long UI spells. Rather, these were intended to exclude claimants for whom the demonstration services were inappropriate. However, we want to examine the degree to which these and other initial screens contributed to the identification of claimants likely to exhaust their benefits.

1. Validity of the Two-Step Selection Process

The analysis presented in this section is based on three key groups. The first group consists of a sample of the claimants who did not pass the initial eligibility criteria. The second group consists of a sample of the claimants who passed the initial eligibility criteria, but whose predicted probabilities of exhaustion were below the threshold level for eligibility. The third group consists of control group members who met all eligibility criteria. Because none of the claimants in any of these groups received demonstration services, we can assess the effectiveness of the eligibility criteria in identifying the appropriate claimants based on their outcomes.

We examine the validity of the two steps in the selection process by comparing mean outcomes among the three groups. The primary outcome of interest is the rate of benefit exhaustion, since this outcome is directly related to the second stage of the selection process, in which claimants were profiled according to predicted probability of exhaustion. We expected the claimants who were excluded from the demonstration using the probability threshold to have a lower rate of exhaustion than those who passed the threshold. We also expected the excluded group to have shorter UI spells. Another important outcome is the probability that claimants returned to their previous employer. Since most of the screens used in the first stage of claimant selection were intended to exclude claimants with employer attachments, we expected the screened-out claimants to have a higher probability of returning to their previous employer. The initial screens may have also had an effect

on mean UI outcomes, but the screens were not specifically intended to identify claimants likely to have long UI spells or exhaust their benefits. Mean outcomes for the three groups are presented in Table B.1.

The combination of initial screens and the probability threshold identified claimants who were likely to spend a long time on UI, and each step appears to contribute to this identification. As shown in Table B.1, claimants who did not pass the initial eligibility criteria had lower exhaustion rates and shorter UI spells than the other two groups. Although the initial screens were not designed specifically to exclude claimants with short spells, they appear to have done so. The claimants excluded from the demonstration by the initial screens have benefit exhaustion rates of 43.9 percent in D.C. and 37.7 percent in Florida. Average UI spells for these groups were 18.5 weeks in D.C. and 13.6 weeks in Florida. In both states, the exhaustion rates and the average UI spells for the claimants excluded by the initial screens were lower than those for the claimants who passed the screens, and the differences are statistically significant at the 95 percent confidence level.

The application of the exhaustion probability threshold further focused the demonstration on a group of claimants with high exhaustion probabilities and long UI spells. In D.C., 58.8 percent of the claimants who passed the threshold (and were therefore eligible for the demonstration) ultimately exhausted their benefits, compared with 47.9 percent of the claimants who did not pass the threshold. The comparable rates in Florida were 45.0 percent exhaustion for those who passed the threshold compared with 40.0 percent for those who did not. The differences in exhaustion rates in both states are statistically significant at the 95 percent confidence level. The application of the threshold also generated groups with average UI spells that were significantly different. In D.C., those passing the threshold had average UI spells of 20.1 weeks compared with 18.6 weeks for those who did not pass. Findings were similar for Florida, where the average UI spell was 15.8 weeks for

TABLE B.1

MEAN UI AND EMPLOYMENT OUTCOMES BY ELIGIBILITY SCREENING STATUS

	District of Columbia			Florida		
		Passed Initial Screens			Passed Initial Screens	
	Did Not Pass Initial Screens	Did Not Pass Threshold	Passed Threshold[a]	Did Not Pass Initial Screens	Did Not Pass Threshold	Passed Threshold[a]
Exhausted UI Benefits	43.9%**	47.9%**	58.8%	37.7%**	40.0%**	45.0%
Weeks of UI Receipt	18.5**	18.6**	20.1	13.6**	14.1**	15.8
Employed with Same Employer	61.2%**	49.1%	51.4%	44.4%**	27.6%*	29.9%

SOURCE: State UI claims records; state UI wage records.

[a]Includes only claimants in the control group.

*Mean outcome for group that did not pass screens or threshold is significantly different than mean outcome for group that did pass at the 90 percent level of confidence.

**Mean outcome for group that did not pass screens or threshold is significantly different than mean outcome for group that did pass at the 95 percent level of confidence.

those who passed the threshold and 14.1 weeks for those who did not pass. These differences are significant at the 95 percent confidence level.

As expected, the application of the initial screens identified a group of claimants who were likely to return to their previous employer. In D.C., 61.2 percent of claimants excluded from the demonstration by the screens who were re-employed had returned to their previous employer. This proportion is significantly higher than for the claimants who passed the initial screens at the 95 percent confidence level. In comparison, 49.1 percent of those who passed the screens but not the probability threshold, and 51.4 percent of those who passed both screens and the threshold, returned to their previous employers. The difference between these latter two groups is not statistically significant. The findings are similar for Florida. Among claimants who were excluded by the screens and subsequently re-employed, 44.4 percent had returned to their previous employer. This rate is significantly higher than for the claimants who passed the screens. Among those passing the screens, those who also passed the threshold had a return rate of 29.9 percent and those who did not pass the threshold had a return rate of 27.6 percent. The rate for claimants who did not pass the threshold was significantly lower at the 90 percent confidence level. This difference may indicate that probability of returning to the previous employer is related to industry, occupation, or the local unemployment rate, which are included in the model.

To summarize the impact of the claimant selection process used in the demonstration, we compare the outcomes for the combined group of ineligible claimants--both those who did not pass the initial screens and those whose predicted exhaustion probabilities were below the eligibility threshold--with the outcomes for eligible claimants. These outcomes, which are presented in Table B.2, confirm that the eligible claimants had longer UI spells and were more likely to exhaust their benefits than the ineligible claimants. In Florida, for example, the benefit exhaustion rate was 6

TABLE B.2

MEAN UI AND EMPLOYMENT OUTCOMES FOR ELIGIBLE AND INELIGIBLE CLAIMANTS

	District of Columbia		Florida	
	Eligible[a] Claimants	Ineligible[b] Claimants	Eligible[a] Claimants	Ineligible[b] Claimants
Exhausted UI Benefits	58.8%	45.9%**	45.0%	39.0%**
Weeks of UI Receipt	20.1	18.6**	15.8	13.9**
Employed With Same Employer	51.4%	55.4%**	29.9%	34.9%**

SOURCE: State UI claims records; state UI wage records.

[a]Includes claimants in the control group who passed both the initial screens and the probability threshold.

[b]Includes claimants who did not pass the initial screens and/or the probability threshold.

*Mean outcome for the ineligible claimants is significantly different from the mean outcome for the eligible claimants at the 90 percent confidence level.

**Mean outcome for the ineligible claimants is significantly different from the mean outcome for the eligible claimants at the 95 percent confidence level.

percentage points higher for the eligible claimants than for the ineligible claimants (45.0 percent compared with 39.0 percent). In D.C., the exhaustion rate was about 13 percentage points higher for the eligible claimants (58.8 percent compared with 45.9 percent). The differences in average UI spells between eligible claimants and ineligible claimants was about 2 weeks in Florida (15.8 weeks compared with 13.9 weeks) and 1.5 weeks in D.C. (20.1 weeks compared with 18.6 weeks). The eligible claimants were also less likely to return to their previous employer.

Finally, although not shown in Tables III.1 or III.2, we found that eligible claimants (those who passed both the screens and the threshold) and ineligible claimants (those who failed to pass either the screens or the threshold) differed along important background characteristics. For instance, in Florida, eligible claimants were more likely than ineligible claimants to be older, female, white, Hispanic, have lower education, and to have higher base wages. In contrast, eligible claimants in Florida were less likely than ineligible claimants to be black. In D.C., eligible claimants had lower base wages and lower weekly benefit amounts than ineligible claimants.[3]

These findings suggest that the demonstration eligibility criteria achieved the objective of targeting the demonstration services to claimants who were likely to have long UI spells and exhaust their benefits. Targeting claimants with long UI spells served at least two purposes. First, the demonstration served claimants who generally faced difficulty in becoming re-employed and therefore may have needed the type of assistance offered by the demonstration. Second, by targeting services to claimants who received substantial UI benefits, the demonstration may have targeted services in a way that offered substantial UI savings if claimants were able to use the services to become re-employed more quickly.

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[3]Weekly benefit amount among ineligible claimants was $264. For eligible claimants it was $212, or 20 percent lower.

Although the eligibility criteria were effective in targeting services to claimants with higher probabilities of exhaustion and longer UI spells, the differences between the groups were not enormous. We were not able to separate claimants into one group in which practically everybody exhausted and another group in which practically nobody exhausted. This is a reflection of the difficulty in predicting UI outcomes based on the characteristics and work experience of individual claimants at the time they filed their initial claim. Even after accounting for the characteristics included in the profiling model, there was a substantial part of variation in exhaustion and UI spells that remained unexplained by the model.

2. Validity of Each Initial Screen

Although the results in Table B.1 demonstrate that the set of initial screening criteria identified claimants who were likely to have long UI spells, exhaust their benefits, or return to their previous employer, it is important to examine the effect of each of the initial screens in this identification process. The initial screens were not designed specifically to identify claimants with long spells and high exhaustion probabilities. Instead, the screens were designed to exclude claimants for whom the demonstration services were probably inappropriate. For example, interstate claimants are no longer living in the state and therefore cannot be served. The other screens were included for similar reasons. Regardless of the various justifications for including each screen, we would like to determine the extent to which each of the screens contributes to the objective of targeting the demonstration to claimants with long UI spells and high exhaustion probabilities.

In the analyses described below, we estimated regression models of UI spell duration and logit regression models of benefit exhaustion using the sample of claimants who did and did not pass the initial screening criteria to identify the impact that each of the initial screens had on selected outcomes. We also estimated logit regressions using as the dependent variable an indicator of

whether claimants returned to their previous employer. The results of these regressions allow us to determine whether certain initial screens achieved the objective of screening out job-attached claimants. Table B.3 shows the estimated effect of each screen, controlling for the effects of each of the other screens.

a. District of Columbia

The findings for D.C. show that all but one of the initial eligibility screens is a statistically significant predictor of at least one of the selected outcomes shown in Table B.3. Although the screens were not designed specifically to exclude claimants with short UI spells, they appear to have done that. Claimants in D.C. with transitional claims, interstate claims, union hiring hall attachment, or whose first payment was a partial payment were substantially less likely to exhaust their benefits and had fewer weeks of UI receipt. For instance, partial first payments were associated with a 29.0 percentage point reduction in the likelihood of exhausting benefits and 1.1 fewer weeks of UI receipt, after controlling for the effects of other screens. Transitional claims were associated with a 19.8 percentage point reduction in benefit exhaustion and 2.6 fewer weeks of UI receipt, while union hiring hall attachment was associated with a 22.2 percentage point lower rate of exhaustion and 1.9 fewer weeks of UI receipt. Claimants in a labor dispute had average UI spells that were substantially lower, by 7.2 weeks, than those for other claimants.

Not surprisingly, several of the initial eligibility screens are significantly related to re-employment with the previous employer. The biggest impacts occurred for the transitional claim, union, and partial first payment screens. Claimants who fell into any of these categories were substantially more likely than other claimants to return to their previous employers. For example,

TABLE B.3

IMPACTS OF INITIAL ELIGIBILITY SCREENS ON UI OUTCOMES

Outcome Variable	Transitional Claim	Interstate Claim	Temporary Layoff	Union[a]	Labor Dispute	Late Payment	Partial Payment	Long Distance
Exhausted UI Benefits (Percent)	-19.8**	-9.2**	-8.3	-22.2**	±	-.3	-29.0**	NA
Weeks of UI Benefits	-2.6**	-1.4 **	-1.5*	-1.9**	-7.2**	0.2	-1.1 **	NA
Employed With Same Employer (Percent)	25.0**	-2.7	-5.8	18.1**	±	-3.2	15.6**	NA
Exhausted UI Benefits (Percent)	-6.2*	NA	-4.0	1.2	NA	-.5	-22.6**	-7.8*
Weeks of UI Benefits	-1.7**	NA	-2.8 **	.2	NA	-1.1**	-3.9 **	-1.7**
Employed With Same Employer (Percent)	25.3**	NA	35.1**	18.3**	NA	6.8**	14.0**	3.4

SOURCE: State UI claims records; state UI wage records.

NOTE: Sample used in this analysis includes claimants in the control group, claimants who passed the initial eligibility screens but not the probability threshold, and claimants who did not pass the initial eligibility screens. Cases are weighted to reflect the different likelihoods that claimants in each group were included in the analysis. Numbers shown are the changes in outcome variables for a one unit change in the initial screens, evaluated at the means of the outcome variables

[a]In Florida, this screen included seasonal workers as well as those using a union hiring hall.

*Coefficient in logistic regression model is statistically significant at the 90 percent confidence level in a two-tailed test.
**Coefficient in logistic regression model is statistically significant at the 95 percent confidence level in a two-tailed test.

± = too few cases to include in the model.
NA = not available in this state.

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among employed claimants, those with transitional claims were 25.0 percent more likely to return to work with their former employer. As expected, each of these screens was a strong indicator that claimants were job- attached and was therefore successful in excluding job-attached claimants from the demonstration in D.C.

b. Florida

Several of the screens in Florida had a substantial impact on at least one of the UI outcomes. Only one screen, partial first payment, had a substantial impact on probability of UI benefit exhaustion, reducing it by 22.6 percentage points. The estimated impacts of the other screens on exhaustion, which include both positive and negative estimates, were no more than 8 percentage points in magnitude and are not statistically significant. However, several screens had a significant negative impact on weeks of UI receipt. The largest impacts occurred for the partial payments and temporary layoff screens, which are associated with 3.9 and 2.8 fewer weeks of UI receipt, respectively. The transitional claim and distance screens were associated with 1.7 fewer weeks of UI receipt.

These results indicate that partial first payments was the most effective initial screen in Florida for identifying claimants who were unlikely to experience long spells on UI, after controlling for the effects of the other screens. All of the other initial eligibility screens, except for union/seasonal worker status, were effective in helping identify workers unlikely to experience long UI spells. Union/seasonal worker status was not, surprisingly, associated with shorter UI spells and receipt of fewer UI benefits. In the future, it may be preferable to separate union hiring hall status and seasonal worker status to determine whether either is associated with decreased use of UI benefits.

Except for the long-distance screen, all of the eligibility screens are statistically significant in predicting whether a claimant returns to work for his or her previous employer. A claimant on

temporary layoff was substantially more likely than other claimants to return to work with the previous employer, with the difference being about 35 percentage points. Claimants with transitional claims, claimants who were attached to a union hiring hall or seasonal workers, and claimants with a partial first payment were also more likely to return to their previous employment. The impacts of these screens ranged from 14.0 percentage points for the partial payment screen to 25.3 percentage points for the transitional claims screen. Finally, late payment was associated with a 6.8 percentage point increase in the likelihood of returning to the previous employer.

3. Predictive Power of the Profiling Model by the Probability of Exhaustion

Another way to determine the effectiveness of the profiling model is to examine claimants outcomes by their predicted probability of exhaustion. If the profiling model was successful in providing an estimate of claimants' UI experiences, then we would expect claimants' outcomes to be related to their predicted probability of exhaustion. In other words, if the profiling model was successful, claimants with higher predicted probabilities of exhaustion should have been more likely than claimants with lower predicted probabilities to exhaust their UI benefits and receive benefits for longer periods. We used the control group together with ineligible claimants who passed the initial screens to investigate this issue. We grouped these claimants according to their predicted exhaustion probabilities and calculated exhaustion rates and the average length of UI spells for each group. The results of these calculations are reported in Table B.4.

In both D.C. and Florida, claimants with higher predicted probabilities of exhaustion generally were more likely to exhaust their UI benefits and to receive benefits for a longer period of time.[4] In

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[4]Different categories of the predicted probability of exhaustion are used in the two states because historically exhaustion of UI benefits has been more common in D.C. than in Florida.

TABLE B.4

MEAN UI OUTCOMES BY PREDICTED PROBABILITY OF EXHAUSTION

Predicted Probability of Exhaustion	Exhausted Benefits	Weeks of UI Receipt
DISTRICT OF COLUMBIA		
Lower than 0.6	47.8%	18.6
0.6 to 0.7	53.2%	19.3
0.7 to 0.8	61.6%	20.5
0.8 or higher	74.8%	22.6
FLORIDA		
Lower than 0.3	36.1%	13.9
0.3 to 0.4	40.5%	14.1
0.4 to 0.5	43.2%	14.6
0.5 to 0.6	43.5%	16.2
0.6 or higher	51.8%	18.7

SOURCE: State UI claims records.

NOTE: Estimates are based on a sample that includes claimants in the control group and claimants who passed the initial eligibility screens but not the probability threshold.

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D.C., for example, 47.8 percent of the sample members with predicted probabilities of exhaustion below 0.6 did actually exhaust their benefits, compared with 74.8 percent with predicted probabilities of 0.8 or higher. In Florida, 36.1 percent of claimants with predicted probabilities below 0.3 exhausted their benefits, compared with 51.8 percent of those with predicted probabilities above 0.6. With respect to the length of UI spells, D.C claimants with predicted probabilities below 0.6 received an average of 18.6 weeks of benefits, while claimants with predicted probabilities of 0.8 or higher received an average of 22.6 weeks of benefits. A similar pattern is observed in Table B.4 for the Florida claimants. These findings suggest that the UI outcomes were generally related, as expected, to the predicted probabilities of exhaustion generated by the profiling model.

4. Validity of Each Profiling Variable

In this section, we examine the impact of each of the profiling variables on the UI outcomes. This analysis identifies which profiling variables helped to identify claimants likely to exhaust their UI benefits. As part of the this analysis, we estimated regression models using the subsample of claimants who passed the initial eligibility screens (but were not assigned to a treatment group) to estimate the effect of each profiling variable, controlling for the effects of the other profiling variables.

a. District of Columbia

In Table B.5, we present findings on the effects of each of the profiling variables in the two states. In D.C., the variables that were positively associated with benefit exhaustion and had statistically significant coefficients include the local unemployment rate, and indicators for prior employment in machinist occupations (compared with professional occupations) and finance, insurance, and real estate industries (compared with service industries). The magnitude of the

TABLE B.5

IMPACTS OF PROFILING VARIABLES ON UI OUTCOMES

Profiling Variables	District of Columbia		Florida	
	Exhausted UI Benefits (Percentage Points)	Weeks of UI Receipt	Exhausted UI Benefits (Percentage Points)	Weeks of UI Receipt
Local Unemployment Rate	2.0**	0.2**	2.5**	0.3**
Job Tenure 3 to 6 Years	1.6	0.7*	-0.1	2.6**
Job Tenure 6 to 10 Years	-1.1	0.2	7.4**	4.9**
Job Tenure 10 Years or More	2.7	0.8	8.8**	4.9
No High School Degree	-4.0	-0.7	7.8**	0.2
Associate's Degree	-0.2	-0.1	-0.4	-0.0
Bachelor's Degree	-6.5**	-0.5	-1.0	-0.1
Master's Degree or Ph.D.	-7.2**	-0.8	0.5	0.3
Clerical and Sales Occupation	2.5	0.7*	2.8*	0.2
Service Occupation	-10.7**	-1.6**	-1.0	-0.3
Agriculture/Forestry/Fishing Occupation	-7.4	1.7	3.0	0.9
Processing Occupation	1.2	3.7	2.4	0.0
Machinist Occupation	14.5*	1.6	1.2	-0.2
Benchwork Occupation	12.3	2.8	4.1	-0.5
Structural Occupation	-1.3	-0.2	-6.6**	-0.9**
Agriculture and Industry	±	±	-6.0	-2.6**
Mining Industry	±	±	15.5	6.6**
Construction Industry	-5.0	-1.0	7.0**	0.2
Nondurable Manufacturing	-7.7	-3.2**	8.3**	1.7**
Durable Manufacturing	-10.7	-5.9*	-8.0	-0.2
Transportation and Utilities	-1.0	-0.0	3.5	0.6
Wholesale Trade	6.6	0.9	0.6	0.4
Retail Trade	0.7	0.1	0.1	-0.4
Finance, Insurance, Real Estate	10.1**	1.7**	6.2**	1.0**
Public Administration	4.1	0.6	13.4**	2.7**

SOURCE: State UI claims records, JSA participant tracking system.

NOTE: Includes claimants in the control group and claimants who passed the initial eligibility screens but not the profiling threshold. Cases are weighted to reflect the different likelihoods that claimants in each group were included in the analysis. Figures shown are the change in outcome variables for a unit change in the profiling variables, evaluated at the means of the outcome variables.

*Coefficient in the regression model is statistically significant at the 90 percent confidence level in a two-tailed test.
**Coefficient in the regression model is statistically significant at the 95 percent confidence level in a two-tailed test.

unemployment rate coefficient implies that a 1 percentage point increase in the local unemployment rate was associated with a 2.0 percentage point increase in the likelihood of benefit exhaustion, other things being equal. The coefficient on the machinist variable suggests that the probability of exhaustion was 14.5 percentage points higher for claimants previously employed in a machinist occupation than for claimants previously employed in a professional service occupation. Similarly, the exhaustion rate was 10.1 percentage points higher for claimants previously employed in finance, insurance, and real estate industries than for claimants in service industries.

Three variables that were negatively associated with benefit exhaustion and had statistically significant coefficients are the indicators for a service occupation, a bachelor's degree, and an advanced degree. The coefficient on the service indicator implies that the probability of exhausting benefits was 10.7 percentage points lower for claimants previously employed in service occupations than for claimants previously employed in professional occupations, other things being equal. The coefficients for the two education indicators imply that claimants with bachelor's and advanced degrees were 6.5 percent and 7.2 percent less likely to exhaust their benefits, respectively, than claimants with only a high school degree.

Generally, the same profiling variables are statistically significant predictors of the number of weeks that a claimant received UI benefits. An increase in the unemployment rate of one percent was associated with a 0.2-week increase in the receipt of UI benefits. Employment in a finance, insurance, and real estate industry was associated with 1.7 more weeks of UI receipt. Employment in service occupations was associated with 1.6 fewer weeks of UI receipt than employment in technical or managerial occupations. Employment in nondurable manufacturing industries was associated with 3.2 fewer weeks of UI receipt than employment in service industries.

These results show that the local unemployment rate, education, industry, and occupation are important predictors of UI outcomes. Although tenure may be an important predictor of UI outcomes by itself, it did not provide much additional help in identifying claimants in D.C. with long spells of UI after accounting for the other variables.

Despite the statistical importance of the unemployment rate in explaining UI spells and benefit exhaustion, the variable has only limited usefulness in targeting services to claimants. Since all claimants in a given local area have the same local unemployment rate, the effect of this variable cannot be used to choose between claimants in a local site. The variable can, however, be used to help allocate resources across local areas, because resources could be targeted to areas with high unemployment rates. However, this type of resource allocation is largely a policy decision that may or may not take into account predictions about local UI spells.

b. Florida

In Florida, several variables had significant impacts on the likelihood of benefit exhaustion. Most of these estimated impacts were positive. For example, a 1 percentage point increase in the local unemployment rate was associated with a 2.5 percentage point increase in the likelihood of benefit exhaustion. Job tenure of 6 to 10 years and of 10 years or more (relative to 3 years or less) was associated with 7.4 and 8.8 percentage point increases in the likelihood of exhausting benefits, respectively. Not having a high school degree was associated with a 7.8 percentage point increase in the likelihood of benefit exhaustion. Relative to service industries, prior employment in construction, nondurable goods manufacturing, finance, and public administrative industries was associated with 7.0, 8.3, 6.2, and 13.4 percentage point increases, respectively. The one variable with a significant negative impact on benefit exhaustion was the indicator for prior employment in a structural occupation, which was associated with a 6.6 percentage point decrease in exhaustion.

Job tenure was positively associated with weeks of UI receipt in Florida. For example, claimants with 10 or more years of job tenure had UI spells that were 4.9 weeks longer than claimants with job tenure of less than 3 years. Weeks of UI benefits were also positively associated with the local unemployment rate, and with previous employment in the following industries: nondurable manufacturing, mining, finance, and public administration. Weeks of benefits were negatively associated with employment in structural occupations and agricultural industries.

These results demonstrate that education, occupation, industry, and the local unemployment rate were consistently important predictors of UI outcomes in D.C. and Florida. Findings from both states also revealed that claimants from the finance, insurance, and real estate industries were likely to have long spells and to exhaust their benefits, while claimants from service occupations tended to have shorter spells. The Florida findings also show that job tenure helped explain UI outcomes in that state, in contrast to D.C., where this variable did not significantly affect UI outcomes.

5. Stability of the Profiling Model

In assessing the validity of the profiling model, it is useful to examine whether using more recent data to estimate the profiling model substantially affects the estimated parameter values, and whether substantially different claimants would have been selected using these alternative parameter values for the profiling model. Before the demonstration was implemented, profiling models were estimated for each state using two years of historical data for the period May 1991-May 1993. Results from these models were then used to assign a predicted probability of exhausting benefits for each claimant who filed for benefits during June 1995-June 1996 for D.C. and March 1995-March 1996 for Florida, and who passed the initial eligibility screens. A threshold probability value was chosen, and claimants with predicted probabilities of exhaustion lower than this threshold were deemed ineligible for participation in the demonstration.

Although we would expect the estimated parameters of the profiling model to change when more recent data are used to estimate the model (e.g., changes in local unemployment and labor market conditions), knowing how much the parameters change when more recent data are used to estimate the profiling model may provide valuable information for identifying the optimal timing for updating profiling models with more recent data. For instance, if the coefficients of the profiling model are stable over time, then the profiling model is clearly valid for identifying dislocated workers at any time. If the coefficients are not stable over time, the profiling model may need to be estimated frequently to remain valid. However, the predictions could still be stable even if the coefficients were unstable, in which case frequent re-estimation would not be necessary. In this section, we consider the implications of updating the models.

a. Updating the Profiling Models with More Recent Data

To examine the stability of the profiling models, we used more recent data collected as part of the demonstration for eligible claimants in the control group and for ineligible claimants who passed the initial eligibility screens. For each state, we randomly selected half of the controls and half of the ineligibles to re-estimate each state's profiling model. The estimated parameters from these updated profiling models were then used to calculate predicted probabilities of exhaustion for the remaining half of each sample.

The parameters from the updated profiling models are shown in Table B.6, along with the parameters from the profiling models employed at assignment. The results show that the parameter estimates from the updated profiling models differ considerably from the parameter estimates from the assignment profiling models. In both states, the size of the effects for the unemployment rate and for education changed substantially from one period to the next, and the ordering of effects

TABLE B.6

STABILITY OF THE PROFILING MODEL

Profiling Variables	District of Columbia		Florida	
	Assignment Model	Updated Model[a]	Assignment Model	Updated Model[a]
Local Unemployment Rate	.1313	.0511*	.0045	.1020**
Job Tenure 3 to 6 years	-.0651	.1127	.5080	.2681
Job Tenure 6 to 10 years	-.0710	-.0506	.8292	.6764**
Job Tenure 10 years or more	.0631	-.0365	.8736	.8499**
No High School Degree	.2309	-.1996	.1543	.3135**
Associate"s Degree	-.1287	.0314	-.1995	.0261
Bachelor"s Degree	-.7905	-.2697	-.1628	-.0357
Master"s Degree or Ph.D.	-.7675	-.2491	-.6900	-.1070
Clerical Occupation	.1959	.3262**	.2322	.1157
Service Occupation	-.3393	-.3964**	.2369	-.0109
Agriculture/Forestry/Fishing Occupation	.6681	.5844	.3953	.2056
Processing Occupation	-.9343	-.2450	.1069	-.1842
Machinist Occupation	.2448	.8146	-.1964	.0781
Benchwork Occupation	.7913	.6212	.0983	.0429
Structural Occupatio	.153	.123	.145	-.3938*
Agriculture Industry	□	□	.3895	-.6536
Mining Industry	□	□	.3686	□
Construction Industry	-.3137	-.2738	-.1227	.2065
Nondurable Manufacturing	.0612	.3287	.0408	.4018**
Durable Manufacturing	.1469	-.2551	.6251	.1233
Transportation	.0626	-.1724	.2625	.3607
Wholesale Trade	.1707	.2551	.1338	.0272
Retail Trade	-.2037	-.0058	.1988	.0631
Finance, Insurance, Real Estate	.6603	.5340**	.4612	.3888**
Public Administration	-.2351	.0986	.2181	.7046**

Source: State UI claims records; JSA participant tracking system.

[a]Includes claimants in the control group and claimants who passed the initial eligibility screens but not the profiling threshold. Cases are weighted to reflect the different likelihoods that claimants in each group were included in the analysis.

□ = too few cases to include in the model.

within industry and occupation also changed considerably. In general, neither state appears to have a very stable profiling model.

b. Changes in Profiling When More Recent Data are Used to Update the Profiling Model

To examine the implications of using more recent data to update the profiling model, we compared the characteristics and UI outcomes of selected claimants as determined by the assignment and updated profiling models. To better approximate the way profiling was implemented in WPRS, we compared claimants in the top quartile of predicted probability of exhaustion as determined by the two profiling models.

The figures in Table B.7 show that in D.C., 29 percent of claimants who were in the top quartile of predicted probability of exhaustion based on the assignment model were not in the top quartile based on the updated model. For Florida, the comparable figure is 36 percent. If services were to be targeted to claimants with predicted exhaustion rates in the top quartile, the figures for Florida imply that about one-third of the claimants assigned services by the assignment model would have been excluded from services by the updated model. The figures for Florida also imply that 12 percent of claimants excluded from services by the assignment model would have been assigned services if the updated model had been used.

These results indicate that significantly different populations would be provided with services under WPRS-style profiling depending on which model is used. Such large changes in the eligible populations may be associated with equally large changes in the characteristics and outcomes of claimants targeted under different profiling models. These changes may also affect the estimated impacts of demonstration services.

To explore these possibilities, we first compared the characteristics of claimants in the top quartile based on the assignment models to claimants in the top quartile based on the updated

TABLE B.7

PROPORTION OF CLAIMANTS IN THE UPPER QUARTILE OF PREDICTED PROBABILITY OF EXHAUSTION USING THE ASSIGNMENT MODEL AND THE UPDATED PROFILING MODEL

	Updated Model			
	District of Columbia		Florida	
Assignment Model	Passed	Failed	Passed	Failed
Passed	71%	29%	64%	36%
Failed	10%	90%	12%	88%

Passed = in the top 25 percent of exhaustion probabilities.
Failed = not in the top 25 percent of exhaustion probabilities.

profiling models. As shown in Table B.8, D.C. claimants in the top quartile based on the updated model were more likely than claimants in the top quartile based on the assignment model to have been female, have greater education and higher earnings, and to have lived in areas with lower unemployment. They were also more likely to have been employed in nondurable goods manufacturing, public administration, and retail industries, and in clerical and machinist occupations, and less likely to have been employed in construction and service industries, and in professional and structural occupations.

In Florida, claimants selected on the basis of the updated model were more likely than claimants selected on the basis of the assignment model to have been Hispanic (and less likely to be white), have lower education and lower earnings, and to have worked in areas with higher unemployment. They were also more likely to have been employed in construction, nondurable goods manufacturing, public administration, and retail industries, and in machinist and benchwork occupations, and less likely have been employed in durable goods manufacturing and finance industries, and in professional, clerical, and structural occupations.

Table B.9 shows the UI and employment outcomes of claimants in the top quartile based on the assignment and on the updated profiling models. In D.C., claimants in the top quartile based on the updated model received slightly greater UI benefits and earned slightly less during the year after filing for benefits than claimants in the top quartile based on the assignment model. On the other hand, claimants in the top quartile based on the updated model were also more likely to exhaust their benefits. In Florida, claimants selected by the updated model were somewhat more likely to exhaust their benefits and have lower earnings than claimants selected by the assignment model, but the two groups received similar UI benefits overall.

TABLE B.8

CHARACTERISTICS OF CLAIMANTS IN THE UPPER QUARTILE OF PREDICTED PROBABILITY OF EXHAUSTION USING THE ASSIGNMENT AND UPDATED PROFILING MODELS

	District of Columbia		Florida	
	Assignment Model[a]	Updated Model[b]	Assignment Model[a]	Updated Model[b]
Characteristics				
Age	37.5	36.8	44.2	45.2
Percent Male	48.9	42.2	49.5	46.3
Percent White	4.4	7.3	61.4	57.8
Percent Black	80.2	76.3	15.2	15.1
Percent Hispanic	3.0	2.3	22.2	25.8
Percent No High School Degree	12.2	5.8	23.8	32.2
Percent High School Degree	57.9	56.7	54.5	44.2
Percent Associate" s Degree	25.3	26.4	11.0	12.0
Percent Bachelor" s Degree	1.8	6.2	10.2	10.1
Percent Master" s Degree or Ph.D.	0.4	2.0	0.3	1.5
Tenure	4.1	4.3	6.2	6.5
Weekly Benefit Amount	$204	$209	$185	$182
Base Wages	$17,433	$18,155	$22,690	$22,142
Local Unemployment Rate	7.3	6.6	6.6	7.3
Industry				
Agriculture	NA	NA	1.9	0.0
Mining	NA	NA	0.4	0.3
Construction	3.4	0.2	2.4	5.6
Nondurable Manufacturing	0.8	2.2	4.4	12.0
Durable Manufacturing	0.4	0.4	13.7	6.7
Transportation	2.4	1.8	8.5	8.1
Wholesale Trade	2.2	2.6	8.6	6.3
Retail Trade	10.6	13.7	7.2	10.4
Finance	15.6	14.8	18.4	12.1
Service	45.9	39.4	15.4	17.0
Public Administration	6.4	14.0	3.7	7.4
Other	0.0	0.0	15.3	14.1
Occupation				
Professional	19.4	10.5	21.4	23.7
Clerical	47.9	68.8	35.4	32.5
Service	2.6	0.8	12.1	11.5
Agriculture	2.0	2.0	1.2	0.5
Processing	0.2	0.1	1.1	1.1
Machinist	2.8	5.0	4.8	8.0
Benchwork	1.4	1.4	4.1	7.9
Structural	12.8	7.9	6.6	2.7
Other	10.8	3.2	10.0	6.4

SOURCE: State UI claims records; state UI wage records.

[a]Includes only claimants in the control group.

[b]Includes claimants in the control group and claimants who passed the initial eligibility screens but not the assignment model threshold. Cases are weighted to reflect the different likelihoods that claimants in each group were included in the analysis.

TABLE B.9

MEAN UI AND EMPLOYMENT OUTCOMES FOR CLAIMANTS IN THE UPPER QUARTILE OF PREDICTED PROBABILITY OF BENEFIT EXHAUSTION, AS DETERMINED BY THE ASSIGNMENT AND UPDATED PROFILING MODELS

Outcome	Assignment Model[a]	Updated Model[b]
DISTRICT OF COLUMBIA		
Exhaust Benefits	64.7%	61.6%
Weeks of UI Receipt	20.8	20.8
Amount of Benefits	$4,188	$4,252
Earnings in First Year	$5,603	$5,432
Worked for Same Employer	42.2%	44.6%
FLORIDA		
Exhaust Benefits	45.8%	48.2%
Weeks of UI Receipt	17.0	17.0
Amount of Benefits	$3,048	$3,020
Earnings in First Year	$9,496	$9,020
Worked for Same Employer	30.5%	31.1%

SOURCE: State UI claims records; state UI wage records.

[a]Includes only claimants in the control group.
[b]Includes claimants in the control group and claimants who passed the initial eligibility screens but not the assignment model threshold. Cases are weighted to reflect the different likelihoods that claimants in each group were included in the analysis.

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Even though the updated model placed different claimants in the top quartile of predicted exhaustion probabilities than the assignment model, the evidence does not consistently indicate that the updated model was better than the assignment model at targeting UI exhaustees and other long-term claimants. Given that the profiling models were only moderately effective in identifying targeted individuals, it is not surprising that updating the models did not lead to dramatic improvements over the assignment models. Moreover, since the updated models used data for current claimants, anticipated improvements associated with the using more up-to-date historical data are likely to be smaller than those found in this analysis. Overall, these results show that using more recent data resulted in substantially different parameter estimates for the profiling models in the two states; however, the model based on the more recent data did not identify substantially different types of claimants as being likely to exhaust their benefits. These results suggest that the profiling model based on the more recent data was not unequivocally superior to the assignment model that was based on less recent data. The findings further suggest that, for these states during this period, it was not necessary to update the profiling model with more recent data. However, more frequent updating may be required when economic conditions are changing rapidly.